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The Relation of Sensation to other Categories in Contemporary Psychology

A Study in the Psychology of Thinking

By

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PREFACE

These pages do not pretend to be a survey of the work done upon the problem of the psychology of thinking, such as has been given to us in Professor Titchener's admirable "Lectures". They are rather an attempt to apply the method of immanent criticism in searching out the implications of the category of Sensation as it appears in contemporary discussions of the problem. This method was wittingly adopted—in spite of limitations which it imposed and in spite of the difficulties which it offered from the point of view of formal presentation—on account of the promise it held of leading the inquirer into the heart of the problem of the psychology of thinking.

If this paper should eventually be found to contain any contribution to the problem, it will be obvious at once that the stimulus which gave rise to it lies in earlier contributors to the subject, and it is a pleasure to acknowledge my indebtedness not only to those whose contributions have been critically examined in these pages, but also to the many others not specifically dwelt upon, among them Marbe, Messer, Orth, Mayer, Kakise, Ach, Schwiete, and finally Binet who grappled persistently with the problem, and thought profoundly concerning it, for many years.

Personally, I am indebted to Professor Oswald Külpe for his many courtesies extended to me while the immediate problem of this paper was forming in my mind during my stay at the University of Bonn; to Professor C. Judson Herrick for his cheerful readiness to discuss questions of neurology that arose persistently during the course of the inquiry; to Professor Harvey Carr and Dr. Stella B. Vincent for the helpful suggestions that grew out of our pleasant conferences. It is to Professor James R. Angell that I am beholden for constant encouragement in the pursuit of this task, and for the inspiration that lies in the ideal of scholarly independence of thought. C. R.

I

It is the purpose of this paper to consider an aspect of psychological inquiry as it has been developed especially by a number of workers both in America and Germany. These latter are commonly referred to as the *Denkpsychologen*, yet it would be misleading to suppose that their work is of significance only for the psychology of the logical processes. Nay, it would affect our whole conception of psychic process and psychic content. When we seek to correlate it with the points of view that are familiar to us in America, we note at first blush that in method it has much in common with so-called structural psychology, whilst the problems which it attacks and the questions which it seeks to answer are conceived in a spirit closely akin to the temper of the functionalist. To elucidate and qualify this statement will be part of the burden of the following pages, and it is the earnest hope of the writer that they may contribute in some measure to finding the common ground of these various "schools" in their pursuit of a common task.

Without further introduction we shall proceed to an examination of the sort of problems the movement in question proposes to itself, and its methods of attacking them. As we go along let us note bit by bit the meaning which the various writers put into the psychological terms that they employ. For by doing this it will be easier to note in how far any differences which may be found ultimately to exist between the various schools, are due to differences in point of view as to the nature of consciousness, or to differences in methods of obtaining and interpreting data. Wherever in this paper we are using quotations from the authors under discussion, we shall, when necessary, use the German or French terms of the original, otherwise we shall seek to give an adequate translation in English, sacrificing, where needful, elegance of diction to accuracy of rendering.

II

Instead of launching upon an historical account of the work on the thought processes in its specifically experimental phases, we shall consider first a treatise by Stumpf, entitled *Erscheinungen und psychische Funktionen,* which partakes primarily of the nature of a theoretical exposition of a consistent point of view—a programme for giving context to problems, rather than a completely fulfilled and grounded system. It therefore lends itself admirably also to our purposes, for it gives a setting in the light of which the discussion of the more specific experimental efforts can be more fully appreciated.

Let us consider what Stumpf alleges he finds on turning to the study of consciousness. For him the total content, the "given" of consciousness, falls into three groups. (1) There are the phenomena, the *Erscheinungen,* which correspond closely to the sensation, the image, and, tentatively, the simple affective elements of current structural psychology. (2) Introspection, for Stumpf, further reveals a consciousness of psychic activities, *psychische Funktionen,* of which we may be directly aware, and the consciousness of which cannot be reduced to sensation and affective elements. Willing, emotion, judging, conceiving, to name only a few, are processes of which we are immediately aware: they are given in the same indefinable consciousness and in the same sense in which sensations and feelings are given. (3) Then there are the relations, *Verhältnisse,* which constitute the third type of given. These relations exist, *a,* between the psychic elements of sensation, image and feeling, which Stumpf comprehends under the term *Erscheinungen*—a word which we shall render in the English as phenomena. These relations are not something that is added to phenomena by our minds through the operation of mental activities upon the sensory or affective content—no, they are conceived by Stumpf to be "given" in consciousness in the same sense in which sensations and feelings are given. They belong to the material with which psychic activities operate, but do not come under the head of psychic activities, nor are they products of these.[49] Another

class of relations, *b,* is composed of those between phenomena and *Funktionen,* and between *Funktionen* themselves.[52] These three categories, then, the phenomena, the psychic *Funktionen,* and the relations, constitute for Stumpf the immediately given of consciousness. We shall have occasion, in another connection, to refer to a fourth category, the "structures" (*Gebilde*), which Stumpf does not, however, regard as being "immediately given" in the same sense as the other three.[65]

For Stumpf the immediately given (*das unmittelbar Gegebene*) is that which strikes one immediately as matter-in-fact (*was als Tatsache unmittelbar einleuchtet*). That which is to make this immediate appeal as matter-of-fact must be such that it can come within awareness. (*Was als Tatsache unmittelbar einleuchten soll, muss wahrnehmbar sein*).[51] It is our purpose to note the implications of this conception of the immediately given and to see in how far Stumpf adheres to it in the upbuilding of his psychological system, especially with reference to his treatment of the presence of phenomena and activities in consciousness. It is the content-character of these two factors, the "givenness" of sensation and affective elements, i. e. of phenomena (*Erscheinungen*), on the one hand, and of psychic activities on the other, that shall engage our attention for the present, and we will leave the discussion of the presence of relational elements in consciousness, which is not an unfamiliar doctrine, for later consideration.

It is hardly necessary to expatiate on what Stumpf may mean by saying that *Erscheinungen,* sensory and affective elements of the content of consciousness, are immediately given. It is simply the structuralistic doctrine that under certain conditions we may become aware of redness and blueness, of the tones that make up a clang, of sweetness, of pressure and cold sensations,—that we may become aware of these as parts of the mass of conscious experience of any moment. His classification of phenomenal elements is as follows: *a,* the sensation contents (*Inhalte der Sinnesempfindung*), including the spatial extensity and distribution of visual and contact impressions,—tentatively also temporal duration and sequence; *b,* memory images (*Gedächtnisbilder*), the 'merely imaged' colors, tones, etc.

Stumpf would leave out of consideration for the time being the classification of the pleasure-pain elements involved in sensory processes, on account of their still questionable status, though he is willing to see them subordinated under the head of phenomena, not as attributes of these, but as a special class.[49] It is not the classification of the phenomenal elements that shall interest us most, but rather their status with reference to the whole of consciousness and their relation to what Stumpf calls the *Funktionen,* that constitute for him another group of psychic givens.

Psychic activities (*psychische Funktionen, Akte, Erlebnisse, Zustände*) are such experiences as the awareness of phenomena, the combination of phenomena into complexes, the formation of concepts, apprehending and judging, desiring, willing and emotional states.[50 52] *Funktion* is here not used in the sense of a result attained through a process, as when we say that circulation is a function of the expansion and contraction of the heart, but in the sense of an activity, of a process, in the sense in which the contraction of the heart is an organic function. It is in the former sense that Stumpf believes himself to be justified in interpreting the term function as used by some American psychologists, such as Dewey.[50] Whether such an interpretation is correct is questionable; yet his own use of the term *Funktion* is brought out by his reference: the *Funktionen* are the activity phases of consciousness, of which we may become immediately aware. In this, Stumpf believes, his use of the term differs from that of Dewey. In order to differentiate the two uses of the term in these pages we shall use the German form when the term is used in Stumpf's sense, and the English form when we refer to the meaning connected with the term by the American functionalists. Limiting ourselves to a discussion of the psychological uses of the term, we note the following with reference to the different shades of meaning connected with it. Both Stumpf and the American functionalists use the term with reference to the activity aspect of consciousness. But where such psychologists as Stumpf seek to understand this activity aspect by considering the psychic life by itself, the functionalist seeks to do justice

to the process aspect by considering consciousness with reference to a biological setting.[5] Stumpf looks inward and there in consciousness is a complex of sensory elements, let us say, which together constitute the object of regard. For Stumpf it is a psychic *Funktion,* an activity of which we may become immediately aware, that is operative in binding together these elements into a figure, a rhythm, or a melody. Or again, given the same object, in terms of the same sensory content, we may at one time take a positive volitional attitude toward it, at another time a negative one.[63] The difference in the two states, then, is not a difference in the sensory content of consciousness, but a difference in the *Funktionen* involved. It is the *Funktion* as it is immediately given in consciousness, that, as a part of the pattern of consciousness, constitutes the difference in the pattern in the two cases. There is, to be sure a complication with other *Funktionen,* says Stumpf, intellectual and emotional, but it is important only to note that it is not the phenomenal content that is different in the two cases, but the *Funktionen* that operate upon that content. With Stumpf, therefore, the category of *Funktion* is a structural category insofar as it is a content among other contents, though he would restrict the use of the term content (*Inhalt*), to the phenomena (*Erscheinungen*), whereas the *Funktionen* are acts, of which we may become aware, which may be perceived by directing attention in "another way". Yet on page 15 of his monograph the *Funktionen* are referred to as contents. And like these others they, too, are "immediately given", though different in kind. In this sense, says Stumpf, there has been a discussion of awareness and perception of psychic *Funktionen* ever since Locke and Leibnitz, to say nothing of earlier thinkers. "These writers deny that the consciousness of seeing can be reduced to memory phenomena accompanying the color phenomena, memory images representing the organ of sight, etc. Still less are such interpretations possible in the case of the consciousness of judging and willing. They are convinced that they apprehend psychic activity in the very process itself, whereas colors and tones are apprehended merely as contents of an act of awareness, (*Wahr-*

nehmungsakt), that is, of a particular class of psychic *Funktionen*. According to this view the phenomenal content and the act are interrelated in a manner still to be described, but are not reducible one to the other. . . . It is to be borne in mind that the assertion of the awareness of psychic *Funktionen* as *Funktionen* does not necessarily include a denial of unconscious psychic *Funktionen*."[53]

Thus Stumpf's conception of *Funktion* has its rise. We have said that he looks inward and there in consciousness he discerns sensory and feeling content on the one hand, and "acts" which operate with these, on the other. Contrasted with this mode of procedure, the so-called functional psychologist looks at consciousness with reference to the life-setting of the individual. He tries to understand what function consciousness serves in the total life-process, as it is represented in the adaptation of the individual form to its environment. He asks: What does a given conscious experience do in the adaptive process, and how does it do it?[6] It is here that his conception of psychic function has had its rise. It considers, say a given image content, from the point of view of the function which it serves in some phase of adaptation involving consciousness. Examples of such phases are experiences like perceiving, judging, willing and emotional states. Introspectively, however, the setting in which the given image appears in these phases of adaptive activity, need not necessarily reveal anything at the periphery of the field of consciousness that is *qualitatively* different in kind from the sensory content of the image which is at the focus of attention and the affective elements associated with it. If the question of the awareness of process should become a problem for the functionalist, say in the case of judgment, he need not necessarily look for it as being given in consciousness in terms of mental texture other than that already known to him in sensation and affection. Having thus briefly sketched the essential features of the two points of view, we shall leave further comparison for the present, and return to a discussion of Stumpf's conception of the relation between two of his categories of psychical givens, the *Erscheinungen* and the *Funktionen*.

III

The difference between *Erscheinungen* and *Funktionen*, says Stumpf, is most striking. "No predicate of the one, unless it be that of duration, can be attributed to the other. Nor do the *Funktionen* possess intensity in the same sense as do tones and odors." They have their own peculiar attributes, "the clearness of awareness, the evidence of judgments, degrees of generality in the case of conception. If in the case of the emotional *Funktionen* there should be found somewhat analogous to the intensity peculiar to sensory impressions, one need not deny it, but then we have to deal merely with an analogy, and not with an intensity in the same sense of the word as in the case of sensation."[54]

Two criteria for distinguishing between *Erscheinungen* and *Funktionen* are given by Stumpf. (1) Each is independently variable over against the other.[56] (2) "No predicate of the one, unless it be that of duration, can be attributed to the other."[54] And we are inclined to find implicit in his exposition a third criterion that appears to have been operative to a certain extent in bringing Stumpf to his distinction between these two types of ultimates: the mode in which they are apprehended. They are apprehended by "different directions of consciousness". The phenomena (*Erscheinungen*) "stand over against us as somewhat objective, that possesses its own laws, a somewhat that we have merely to describe and acknowledge". The *Funktionen*, however, we are told, are given us by "another direction of consciousness".[57] While Stumpf does not enlarge upon this third criterion, viz., mode of behavior under introspection, we still find it implicit in his statement that the *Funktionen* are apprehended by a "direction of consciousness" other than that by which the phenomena are apprehended.

IV

Stumpf believes that he can postulate on introspective grounds the possibility of independent variability of either the *Erschein-*

ungen or the *Funktionen,* without a variation in the one being necessarily accompanied be a concomitant change in the other. It will not be unprofitable, we believe, to follow him a little way while he is trying to make his point, for it will help us somewhat in understanding how he comes to believe that he is justified in regarding the *Funktionen* as being immediately given, and also to understand just what phases of consciousness he would subsume under that concept. Let us consider first his instances in which he believes a change in *Funktion* to occur while the *Erscheinungen,* the sensory content, remains unchanged. He first takes up a case in which the *Funktion* of awareness is involved. Awareness is apparently taken by Stumpf in the sense of "simple apprehension" the most primitive of psychic *Funktionen,* the process "by which parts or relations are precipitated out of the undifferentiated chaos of phenomena".[57] It is the *Funktion* that precedes the judgment, whether implicit or explicit. "To be sure," he continues, "there usually goes with it an instinctive positing of the part noted, and later it is often accompanied also by a conceptual judgment concerning the existence of the part or relation." The process of being aware (*Wahrnehmen, Bemerken, Notiznehmen*), is for Stumpf to be differentiated carefully from the *Funktion* of judgment and the "instinctive positing" just mentioned. Awareness of phenomena of the first class, of sensory content, he calls sensing (*Empfinden*), awareness of phenomena of the second class, of images, he calls imagining (*Vorstellen*).[57]

"Our thesis," says Stumpf, "applied to the case of sensory awareness, asserts that in the transition from being unnoticed to being noticed there need not necessarily be a change in the phenomena, in the sensory content, itself. That which changes is essentially of the nature of *Funktion,* of process. The transition, putting it figuratively, consists in an amassing of consciousness with reference to some part of the phenomenal content. Thus when one tone of a chord is singled out, there need not be a change in the chord as phenomenal content. What I apprehend at first as an unanalysed clang, then as an analysed one, remains what it was; so too the unified impression of some article of food, in

which later I note somewhat of sweetness, somewhat of sourness, perhaps also an odor and a temperature quality; so too the dermal sensation which is later analysed into pressure, cold and pain sensations,—these remain what they were. And it is not merely the objective stimuli and the psysiological processes, but also, I believe, the subjective phenomena (*Erscheinungen*) that may remain the same, unchanged."[59] "To be sure," he continues, "in most cases in which we say that a sensory impression appears clearer, more distinct, and more 'transparent' with reference to the total ordering of its contents, than a moment ago, there can be shown numerous changes in associated imagery. . . ." There is a quickening of the apperceptive process. A second view of a picture allows it to be taken in with fewer and shorter stopping points. The muscular sensations are reduced at least with reference to the duration attribute. But not all cases, he believes, can be thus explained. Among them those just cited above. Likewise the following: Coming from the theatre, rapt in thought, we are aware in a vague way of the row of brilliant street lamps, or of the strokes of the bell tolling the hour. We now turn our attention directly to the lights, or to the further strokes. "We will have to say to ourselves," continues Stumpf, "that just a moment ago there also were lights and auditory impressions of the same kind and in the same spatial or temporal relations, incidentally also of the same intensity as those of which we are now aware." What has changed in all these cases is not the sensory content, according to Stumpf, but the *Funktion*. Since we saw that the peculiar attribute of the *Funktion* or process of awareness is the degree of clearness, we must infer that it is that which has changed in all these cases. We recall that by way of definition of this change, Stumpf had said that "the transition, putting it figuratively, consists in an amassing of consciousness with reference to some part of the phenomenal content". It would appear that the factor in conscious experience which is the basis for Stumpf's judgment as to the immediate givenness of the *Funktion* in the case of awareness is just this amassing of attention and holding it there. This much for clinching in a concrete

case the content of his idea of one of the *Funktionen*. We shall revert to this anon, but for the present let us consider in what sense the phenomenal elements constituting the presentation may be said to be the same or similar in the mental state before attention is directed to the lights and in the state in which it is thus directed.

V

In support of his position Stumpf refers to a paper by A. Marty which contains a criticism of James.[58] The passage in James to which Marty refers has the following citation from Stumpf: "And when, after successfully analysing this total, we call it back to memory, as it was in its unanalysed state, and compare it with the elements we have found, the latter (as it seems to me) may be recognized as real parts contained in the former, and the former seen to be their sum. So, for example, when we clearly perceive that the content of our sensation of oil of peppermint is partly a sensation of taste and partly of temperature."[29] Commenting on this, James says: "I should prefer to say that we perceive that objective fact, known to us as the peppermint taste, to contain those other objective facts known as aromatic or sapid quality, and coldness, respectively. No ground to suppose that the vehicle of this last very complex perception has any identity with the earlier psychosis—least of all contained in it." Closer consideration of these two statements will bring us face to face with the parting of the ways between so-called structuralism and functionalism, and will also make clear to us how divergent are the two attempts to do justice to a completer understanding of the activity side of consciousness that are represented by such psychologists as Dewey and Angell on the one hand, and by Stumpf on the other. It is hoped that this consideration, together with its bearing upon a discussion of the experimental investigations of the thought processes, which is to

follow, will be a sufficient justification for broaching in this connection the subject of the psychologist's fallacy and of the immediately given.

VI

We saw that according to Stumpf the immediately given is the "phenomena and *Funktionen* and the relations immanent in these, of which an individual is momentarily conscious", i.e., the psychically immediate. We have no reason to believe that a psychologist of the temper of James could not agree to this definition, at least in so far as the given is defined as that of which the individual is momentarily conscious, or the psychically immediate. The difference therefore must lie in the use to which the data thus gained in the form of the subject's judgments regarding the content of his immediate experience are put and in the interpretation that is placed upon them. James' contention is that the peppermint experience is a totality and has nothing in common with the experience in which consciousness analyses oil of peppermint into a complex of stimuli mediating taste and temperature sensations. That analysis has been made under conditions in which attention is focussed on the content as *Erscheinungen* and yields results in a rather elaborate judgment as to the character of the "object" presented. (We can here leave aside the question of James' use of the term, "object" and "objective".) The two experiences are quite dissimilar and the latter cannot be said to be contained in the former. James here refers to the analytic experience as a whole—in the terms of Stumpf not merely to the *Erscheinungen* which constitute the "object", but also to the *Funktionen* which may be directly involved in giving the immediate experience of peppermint-analysed-into-its-elements.

Taking each case by itself we may say that in the unanalysed state that which strikes as immediate matter-of-fact (*was als Tatsache unmittelbar einleuchtet*) is to be couched in terms of

the judgment, that within the larger setting which it had with reference to some activity, the part that was singled out and noted was a unitary somewhat; the peppermint, with a pleasurable or painful feeling tone, mayhap also some somatic and visceral sensations. This much is the immediately given—a description of the immediate psychical content. It is that which the subject reports. So much for the first phase of the experience, in which the object is "given" as a whole, unanalysed. Then follows the analytic phase, or the one in which the object is apprehended at the focus of attention. The individual now reports that he can discriminate two sorts of sensations: taste and temperature. This statement, plus anything he may have to say concerning his awareness of concomitant process, whether sensations connected with the accommodation of the sense organ, or *Funktionen* if he employs that category, or other phenomena at the fringe,—these will constitute the psychological data of the immediately given of the second experience. We do not pretend that this is an exhaustive statement of all that a careful observer might report as the immediately given in the two experiences. But it is by means of such judgments only that the psychologist can in any sense be said to get at the content that is immediately given in any consciousness other than his own. It is only in this way that the immediately given in the character of "mind-stuff" can be made the *starting point* for a body of psychological knowledge dealing with human consciousness.[68] We now ask: What may the investigator do with the data thus gained? Stumpf claims that he is entitled to read back into the first experience the "content" of the second experience. There is nothing, to be sure, that would militate against his right to do this if he so desires. And if by doing so he obtains a system of knowledge that serves his purposes and that may serve the purposes of others, he is thereby justified by his procedure, so long as in doing so he is not introducing an element of immanent inconsistency into his system.

Yet we believe that this latter thing does happen. In what sense can that part of conscious content abstracted from the second experience be said to be "recognized as real parts con-

tained in the former"? The abstracted "elements" are immediately given as part of another, the second experience, and that section of the content of the second experience therefore cannot be said to be identical with the content of the first experience or any part thereof. If not that, have we any immediate evidence that they are identically similar? If we take immediate evidence to mean the record of immediate judgments in the sense defined above, then this question, too, must receive a negative reply. Even Stumpf can do no more than tell us that "we will have to say to ourselves that just a moment ago", while we were lost in thought in coming down the lighted thoroughfare, "there also were lights and auditory impressions of the same kind, and in the same spatial or temporal relations, incidentally also of the same intensity as those of which we are now aware". This statement, however, is not of the nature of an immediate judgment passed upon the immediately given content of the first experience, for such could only have been obtained by stopping our man lost in reverie and requiring him then and there to tell us concerning the character of sensory content as it was marginally present in consciousness. Stumpf himself tries to subsume his judgment just quoted under the head of direct comparison, for he is apparently aware that a doctrine so important for his theory as this is, ought to rest on some kind of evidence. We do not here cavil at his argument that the term "direct comparison" ought not to be restricted to those cases in which the two contents to be compared are simultaneously present to consciousness during the act of comparison, but that it should be made to include also those cases of comparison of a present content with another that is just passing, or of one just passing with one that preceded it but is still lingering in consciousness.[61] We would, however, suggest that the conditions under which this method of direct comparison is to be employed, be more rigidly defined, in order that we may have a criterion for the validity of the evidence which is yielded by this method. The evidence derived from the illustration cited in Professor Stumpf's treatise does not appear to be altogether unambiguous. Reverting to the case of the lights

and the strokes of the bell tolling the hour while we are coming from the theatre, lost in thought, we are told that we can escape the interpretation that the two contents are alike only by making special artificial assumptions, e.g.: that in the reproduction of the content just past for purposes of comparison with the present content, there is in the process of reproduction a change or an assimilation to the standard of the present content.[60] Why such an objection should appear particularly far-fetched and "artificial" is not immediately patent to us. That such an assimilation of past experience to the standard of present experience frequently does occur, no one will doubt. We know that the lights of our city streets possess fairly uniform candle power, that the tolling of the bell in the steeple, too, is fairly constant in timbre and rate of stroke,—what then would be more natural at first blush than to infer that the experience of just a moment ago must have been practically (there is much virtue in this expression of our everyday life) the same as it is now that we are attending to the objects directly. (At this point Titchener might well warn us against what he has called the "stimulus error.") And even when the judgment is not based on mediate experience as just described, but an attempt is made to base it directly on a comparison of the two contents by the method discussed above, it would be preferable to have the two experiences called out under somewhat different conditions than is the case in the illustration, i.e., we would ask for experimental conditions that would minimize the suggestive influence of the content given in the state of focalized attention.

A thorough-going experimental psychology would demand that every aspect of the sensory experience be compared in the two cases. What is the difference of the content mediated by the same objective stimulus under conditions of focal and of marginal apprehension? How do the sensations compare with reference to their various attributes? Do they differ at all in quality, or in duration, or in extensity, or in intensity? And then the further question arises whether these are the only respects in which the sensations may differ. Only after all of these are answered are

we entitled to conclude that on the basis of immediate introspective evidence the two contents must be regarded as being identically similar. Lacking experimental evidence on some of these points, we turn to everyday introspection and to the difficulties which this question has caused in some of the theoretical discussions, for a formulation of the problem. Here we note that certain doctrines have become so thoroughly incorporated into the psychological point of view, that they inhibit effectually the rise of some of the questions indicated above. The doctrine of specific energy apparently precludes debate as to the question of the sameness of quality, so long as the objective stimuli and the nerves stimulated remain the same. Duration, too, so far as the immediate sensation is concerned, as over against its influence upon future psychic experience, would be regarded as the function of the same factor in both cases, i.e., of the application of the stimulus subject to the conditions of psychological fatigue. We would question, however, whether the immediately given duration for consciousness would necessarily be the same. Without raising the issue of a possible difference in behavior of intrinsically pleasurable and intrinsically painful sensory processes as regards subjective duration under the two conditions, of being attended to on the one hand, and not being attended to on the other,—we would ask whether the attention reaction does not often contain within itself the conditions for prolonging the objectively measured time of what subjectively is experienced as a uniformly continuous sensory process? This problem is perhaps most fascinating in the field of cutaneous and auditory sensation. In the case of the bell tolling the hour, there is a possibility for the last chime to linger on in consciousness in a way it does not do under conditions of inattention. Under such circumstances the ear wooes the sound, reaches after it, as it were, as it retreats and gradually envelopes itself in nothingness. When we turn to the attribute of extensity, orthodoxy again inhibits any tendency to dissent from the statement that the two states exhibit no difference of sensory content in this respect. The moment, however, that we come to the question of intensity,

we note a more pronounced lack of unanimity. At this point we can find some experimental material that is more or less germane. If we take the term broadly enough, most of these experimental attempts may be subsumed under the head of distraction tests. Most of this work is mentioned by Titchener in his "Lectures on the Elementary Psychology of Feeling and Attention", and its trend is in general to show that within a certain range at least attention does tend to increase the intensity of the sensory content.[91] Stumpf himself admits this much in a later monograph, *Zur Einteilung der Wissenschaften,* where he says: "It does happen, of course, that at times the *Funktionen* retroactively bring about a change in the *Erscheinungen,* as when in the case of concentrated attention the intensity of a very weak sensory or image content is raised to a certain degree."[69] This would in a certain sense be equivalent to admitting the validity of our objection, if it were not for his immediate qualification following the statement just quoted: "But in general such retroaction does not occur, and when it does occur, it is always within the limits of the possibilities prescribed by the nature of the *Erscheinungen.*"

We find, then, that there is some question whether we are justified in asserting unqualifiedly that the sensory "content" of the two types of experience mentioned by James and Stumpf, is identically similar as regards the usually recognized "attributes."

VII

Having touched upon the question with reference to the usually recognized attributes of sensation, we may go on to ask whether these are the only directions in which the sensory content might possibly differ in the two states. Here we come upon one of the most vexing of the moot questions of psychology, the question of clearness or vividness as an attribute of sensation. All writers appear to agree that if there is an independently distinguishable aspect of conscious experience that is to be designated by that name, it is to be noted in connection with that phase of

consciousness which is known as attention. Stumpf, we saw, makes clearness an attribute, not of the sensory content, but of the *Funktion* of awareness. The amount of consciousness that is involved in my being aware of the bonfire outside, may vary from almost none, when I am only marginally aware of it, to almost all, as when I sit fascinated, as it were, by the leaping tongues of flame and the mystic clouds of smoke. The difference in the two cases, we have already seen, at too great length, perhaps, lies for Stumpf not in the sensory content, but in the mental activity that is operating upon that content, an activity of which we may be distinctly aware and the attribute of which is just this degree of clearness. When we turn, however, to the pages of a psychologist such as Titchener, we read the following: "Whatever attention is, it must be described in terms of mental processes, sensations and images and affections, and explained by reference to its physiological conditions."[77] Titchener, therefore, proceeds to make clearness an attribute not of a mental activity, but of one of the elements, viz., sensation. "Clearness is the attribute which gives a sensation its particular place in consciousness: the clearer sensation is dominant, independent, outstanding, the less clear sensation is subordinate, undistinguished in the background of consciousness."[76] A sensation is clear when it "is at its best, when it is making the most of itself in experience. Clearness is an intensive attribute, in the sense that it shows degrees of more or less: but it is altogether different from intensity proper."[78] It is important for us to note only that for Titchener clearness is regarded as an attribute of sensation and that it is "altogether different from intensity proper". In trying to make clear this distinction he introduces a quotation from Wundt. He writes: "In the first place, there can be no doubt of the independent status of clearness as sensation attribute. As Wundt says: 'Klarheit und Stärke der Eindrücke sind durchaus von einander verschieden'; 'das Klarer- und das Stärker-werden eines Eindruckes sind . . . subjectiv wohl zu unterscheidende Vorgänge'."[90] The citation from Wundt is correct; not so, however, the interpretation that Titchener puts upon it. For

when we read the context in Wundt, we come upon the following: "Da die Stärke der Empfindungselemente einer Vorstellung auf die Klarheit einen zweifellosen Einfluss ausübt, so sind nicht selten beide Begriffe mit einander vermengt oder sogar für identisch gehalten worden. Streng genommen kann aber immer nur von der Stärke der Empfindungselemente, nicht von der Stärke einer Vorstellung die Rede sein, da in diese meist Empfindungsinhalte von sehr verschiedener Stärke eingehen. Umgekehrt dagegen sind Klarheit unde Deutlichkeit ausschliesslch Eigenschaften der Vorstellungen, die auf Empfindungen nur übertragen werden können, wenn diese als Vorstellungsbestandteile gedacht werden."[109] In other words, Wundt does not make clearness peculiarly an attribute of the sensation or image element, but an attribute of the complex percept or *Vorstellung*. Wundt is quite explicit on this point, and in the very context from which Titchener takes the quotation, Wundt repudiates the interpretation that is put upon it. But the essential thing for us is that Wundt does note the fact of clearness and that he makes it an attribute of content.

Here we have examples of three different points of view regarding the fact of clearness as a factor of conscious experience. All three agree that introspection reveals the fact of degrees of clearness as an attribute of some phase of consciousness, and that the higher and highest degrees of clearness accrue to states of consciousness under conditions that are technically called "focalized attention". But they differ as to the phases of conscious experience of which clearness is to be regarded the attribute. Titchener makes clearness one of the attributes in terms of which sensation and image elements are to be described. Stumpf makes it the attribute of one of his psychic *Funktionen,* the attribute by which the *Funktion* of awareness is presumably revealed to us as "immediately given". Then we come to Wundt. Like Stumpf, he too believes that we may become directly aware of psychic activities: "Alongside of the going and coming of percepts and ideas (*Vorstellungen*) we are now and then more or less distinctly aware of an inner activity that we call attention."[108] Yet unlike

Stumpf, Wundt would still reduce this consciousness of psychic activity to elements of sensation and affection. But clearness is not an attribute of this complex of elements that taken together go to constitute our consciousness of activity; nor is clearness for him an attribute of the sensation and image element as it is for Titchener; it is rather one of those "new attributes, peculiar to the compounds themselves", that "always arise as a result of the combination of these elements".[106] "Since clearness, obscurity, etc., . . . always arise from the interconnection of psychical compounds, they cannot be regarded as the determinants of psychical elements." We have therefore before us a case of agreement as to the "immediate givenness" of the aspect of clearness in conscious experience, but disagreement as to the particular phase of conscious experience of which it is to be regarded the attribute or determinant.

VIII

Is there any way in which we can gain vantage ground whence we may see how these differences arise in the three psychological systems? One way, it would seem, may prove fruitful. That is, attempting to attain to an appreciation of the immanent relationship of the psychological categories within the various systems. That will obviate an exposition anew of the ramifications of the problem of mental analysis as a scientific method. We will examine instead the precipitants of what various writers regard as legitimate application of this method. Let us consider first various implications involved in Titchener's exposition. For him the "given" is the concrete experience, e.g., the square and the melody. "Our psychological task is to analyse these given perceptions, to discover their elements, and to formulate the laws under which elementary processes combine. That done, we can write for 'square' and 'melody', 'these and these elements connected in these and these uniform ways', and we can go on to search for physiological conditions. We have solved our

problem in analytical terms; we have not first defined the terms, and then put them together to produce something that was not contained in the definition."[81a] Titchener is also fully aware of the relation of his sensation elements to the percept: "The elements are . . . the result of analysis; the perceptions are the original things, and the sensations are found in them by observation; perceptions are given us, and we discover that they are analysable. Misunderstanding here is fatal to the student of psychology, for it means misapprehension of the central psychological problem."[79] The mental elements, processes "that cannot be further analysed by introspection", of which sensations constitute a class, "are simple . . . in the sense that they are mental experience reduced to its lowest terms; but they are still real processes, still actual items of mental experience. Hence, like the chemical elements, they show various aspects or attributes, —present different sides, so to speak,—each of which may be examined separately by the psychologist. It is by reference to these attributes that introspection is able to classify them under different headings".[75] The mental element must be defined by "an enumeration of its attributes".[97] Titchener is furthermore at pains to have us know that the elements of sensation, feeling, and image, are not to be conceived of as static things, but as processes.[71] They melt and fuse and flow into one another as they occur in the stream that we call our consciousness. Yet with all this insistence on the process character of consciousness, Titchener nevertheless reverts ever and anon to the use of analogies taken from physical science which are not always the happiest, if his intention is to emphasize unambiguously this process-character.* Whatever content the chemist may put into his concept of chemical element, one thing there is that is not a part of its meaning, it does not signfy a process. The concept of "process" implies a correlative one of a structural system of parts or elements which act and react upon one another. It is this interaction within a

* Cf. Titchener, A Textbook of Psychology, 46: "They (the elementary mental processes) must remain unchanged, however persistent our attempt at analysis." See also above § 11.

structural system that constitutes a process. In the mind of the chemist the elements are abstractions, to be sure, but the essential *Merkmal* of this abstract concept is that of a relatively fixed form of matter that behaves thus when it is brought into interaction with certain other elements, and thus and so when it comes into interplay with still other forms or combinations, under these and these conditions. And it is this interaction between the various elementary forms of matter that constitutes a chemical process. But for the chemist the abstract element which he chases through these various processes remains a relatively fixed substrate, a "form of matter". Now it may come to pass, however, that a physicist comes along and takes one of the simple substances of the chemist into his own laboratory, and now, for the purposes of the physicist, it may become within itself, without reference to other substances even, a highly complex affair. It may become a closed system of forces or of atoms or what not, which act and react upon one another in certain definite ways. In other words, there are processes going on within, and the physicist by his analysis has gone beyond the analysis of the chemist. What is more, this anaylsis is not altogether merely a matter of theory, but is paralleled by some empirical evidence. For the chemist, however, such an analysis is irrelevant; for him the element remains an ultimate somewhat, which by interaction with others, gives rise to various chemical activities or processes, in the course of which the element may discover various attributes or properties that are brought out by varying the conditions under which it is brought into interaction with the other elements. It is considerations such as these that lead us to believe that the analogy of the chemical element is an unfortunate one if our meaning is that the simplest analysable content of consciousness is essentially a changing, fluid somewhat. Change and flux are the result of interaction of the elements, under varying conditions of heat, of pressure, etc., but these processes are not themselves the elements. To refer to a phenomenon now as an element and now as a process implies a shifting in purpose and in point of view, as in the case of the chemist and the physicist cited above. If the

element is to be considered as a part, an irreducible, ultimate content of consciousness, then it cannot at the same time be regarded as a process, for a process implies the interrelation of a number of elements or factors. The concept of element is, or formerly was, a structural category, whereas that of process is a functional one.

IX

But to leave the question of nomenclature—for we are here interested chiefly in the manner in which the attributive aspects of experience have been singled out and in the method by which they are "attributed" to the various categories, i.e., how do the "attributes" come to constitute the element?—We next note that affections, ideas and sensations are the "results of analysis".[81] They are the simplest, rock-bottom forms of "real", "actual" experience that analysis will divulge. As real components of consciousness they reveal to us certain aspects or attributes, just as do the chemical elements.[75] It is in terms of these attributes that a sensation is to be defined, says Titchener, and the sensation is not something over and above the sum of these attributes. His position is further similar to that of Külpe, who says at the time of writing his text: "These attributes are characterized (1) by their inseparability from the sensation . . . (2) Further, the nullification of any of the attributes involves the disappearance or cessation of the sensation."[32] We may now ask: What is the method by which these attributes of the element are determined? Titchener tells us that the element is not further analysable by introspection. On the other hand we are told that the element presents different aspects or sides, called attributes, that can be separately attended to. Thus attended to they are discriminated, and what is this other than further analysis? Is it other in kind than that which yielded the elements? And are the precipitants of this further analysis of another sort?

This question has been touched upon, among others, by Tal-

bot,[70] Washburn[98] and Calkins.[23] Talbot, writing on "The Doctrine of Conscious Elements", points out that "the modern theory lays particular stress upon the fact that the psychological element is an elementary *process*", and that the "criterion of a psychological element is irreducibility". And yet, coming to discuss the attributes Talbot says that "it is a fact that the work of analysis does not actually cease when they (the irreducible ultimates) are discovered . . . there is need for a second process for the purpose of determining the properties of our elements . . . In the first analysis we passed successively from one process to another, finding in each new stage the explanation of the more complex one which preceded it. When we have at length reached a process which we cannot explain by means of another *process,* our regress is finished, our element is discovered. Whatever analysis may now be possible, will be entirely distinct from the first, and will in no way affect its claim to be complete. The attempt to find an explanation for our process in something else than *process,* the effort to go behind our ultimate in order to explain it, would but repeat the fundamental error of the doctrine of the faculties." Yet the writer has failed to show why the precipitant of the last analysis, the "attribute", is not entitled to be called a process just as much as are the percept and the sensation, which, we take it, constitute the earlier precipitants in the "regress". It is simply asserted that the second analysis is entirely distinct from the first one, *i.e.,* from the analysis by which the sensation is abstracted from the percept, and presumably also from that by which the percept has been abstracted from the total experience of the moment. (This would seem to be implied by the use of the term "regress".) An immanent criticism of the position just noted is impossible, however, for nowhere in the paper is there a direct definition of the conceptions of "analysis" and "process". We take the word "distinct" in the sense of separate process of analysis, rather than in the sense of a different kind of analytic process, for the writer says, "In the first analysis we passed successively from one process to another, finding in each stage the explanation of the more complex one that pre-

ceded it." From this it appears that the writer would not hold that the three analytic processes that we mentioned represented different methods of analysis. The distinction, therefore, must lie in the precipitants, that is. in the percept, the sensation, and the attribute. The percept is a complex process. The sensation is a simple process. It is an "ultimate". The attributes are not processes, they are merely "attributes of our elements". In calling the element a process, Talbot believes that its "functional nature" is being emphasized. From this it would appear that "functioning" is conceived of as being the essential *Merkmal* of "process" as a psychological category. Now it may well be that what analysis brings to light when it discovers what Talbot and Titchener designate as an attribute, may be the functionally determining factor in many of our conscious reactions: e.g., intensity of a tone rather than any other of its "attributes". Intensity functions frequently, just as does quality, or a complex stimulus pattern, in evoking its own peculiar specific reactions, and if this be admitted to be tantamount to ascribing to it a "functional nature", then the intensity, as an "attribute", has the same claim as the sensation, as an "element", to be designated a "process". If, therefore, intensity be conceded to possess functional value in its own right in determining some of our conscious reactions, Talbot cannot on that score deny it the right to be called an elementary process. Also, it appears to satisfy the criterion of irreducibility. The only question that could arise is this: Has it been abstracted from the complex mental state by a legitimate process of psychological abstraction?

Calkins, in a paper on the "Attributes of Sensation", commenting on the position of Talbot, says: "either the sensation has attributes, but then it is complex, no element and has lost its excuse for psychological being; or the sensation is an irreducible and unanalysable element, but then its simplicity is absolute, not to be trifled with, and not to be explained away by reference to any second process of analysis into elements, which yet are not elements, but only 'attributes', 'aspects' or something equally vague and meaningless." Consequently Calkins avers that inten-

sities and qualities "take their places among the distinguishable elements of consciousness", for "if abstract irreducibleness and distinctness be seriously maintained as the sole criteria of the psychic element, analytic psychology has no place and no use for the 'attribute' of sensation". In other words, Calkins affirms that on the criterion of unanalysability it is the "attribute" that should be regarded as the element in consciousness. We have, therefore, two different conclusions as to the psychic ultimates based on the criterion of irreducibility.

Washburn, in a paper on psychological analysis, points out that such differences are very likely to arise so long as our conception of analysis is not carefully defined. Of purely "psychological" methods she distinguishes two: "(1) the psychological method of calling mental phenomena elementary because they are the simplest phenomena that, being independently *variable,* may be attended to separately; (2) the psychological method of calling them elementary because they are the simplest phenomena, that, as capable of being *experienced apart* from each other, may be attended to separately." Calkins' element is a result of the first method; Titchener's is a result of the second.

X

Such being the method by which the sensation is obtained as a psychical element, we find that Külpe's statement, quoted above concerning the nature of the attributes and their interrelations, is quite in accord with Washburn's analysis. And we find that Titchener's statement accords with that of Külpe. For Titchener, then, the sensation element is the sum of all its attributes, and not a something over and above these. "The annihilation of any attribute carries with it the annihilation, the disappearance, of the sensation itself."[82] It is the being given together, therefore, of the attributes that constitutes a sensation a "real process", an "actual item of experience". "If a sensation is to exist, it must come into being with all its attributes."[83] Since an attribute

cannot exist in consciousness without the others, it cannot lay claim to being an independent element of experience; since when one is present, the others, too, will be discovered, and since they somehow constitute a unitary item of experience, the complex is designated as the element. Yet the question may arise whether their unification or incorporation into a single item of experience is a function of their peculiar psychic character, or whether it is not rather a function of the habit of objective reference. If the former is to be the case, then in every fusion of sensations of different departments of sense, introspection would always unambiguously connect up the intensity component of the experience with its proper quality; yet it is still a moot question whether this is always the case. If, on the other hand, the uniqueness and irreducibility of the sensation element is a function of the habit of objective reference to a stimulus acting upon a particular sense-organ, then too it cannot be regarded otherwise than as a derived, rather than an inherent, characteristic of the element. For the present, therefore, we can mean by the term "inseparability of the attributes" nothing more than a condition for their entrance into, and exit from, consciousness; but nothing for certain concerning their behavior while they are in consciousness. It still remains a task for experimental psychology to determine whether or not the incorporation of the "attributes" into a unique item of experience, a particular sensation, is an 'irreducible' fact of consciousness.

It may not be amiss to discuss at this point the relative importance of the various attributes, which would lead up to a consideration of the relation of the attributes *inter se*. If we turn to some of the textbooks of psychology we very often run across a statement to the effect that of all the various attributes that the different writers ascribe to the sensation element, quality is somehow the most important of the lot. It is the "body" of the sensation, so to speak, and so soon as we have passed the introductory pages of preliminary definition, we find many of the writers falling into the way of speaking as if the quality *were* the sensation. Thus Külpe: "Im algemeinen lassen sich alle diese Eigen-

schaften variieren, und es beruht darauf die Möglichkeit ihre Gesetze im Einzelnen festzustellen. Nur spielt auch in dieser Beziehung die Qualität eine eigenthümlich Rolle. Eine Aenderung der Qualität ist mit einem Uebergang zu anderen Empfindungen identisch, während eine blosse Aenderung der übrigen Eigenschaften bei gleichbleibender Qualität scheinbar dieselbe Empfindung fortbestehen lässt. Auch hierin zeigt sich dass die Qualität mit dem Wesen der Empfindung auf das Engste verwachsen ist. Sie representirt gewissermassen gegenüber den anderen wechselnden Eigenschaften den festen Kern der Empfindung."[33] It looms large in its importance for experience, and is singled out and attended to while the other attributes, like mute Cinderellas, perform their humbler functions unobtrusively in the background. Yet here again, it is a question whether this advantage possessed by the attribute of quality must be accepted as an ultimate fact testifying to a unique position of quality as over against the other attributes, specifically as over against intensity. It may well be that the usual function of sensory consciousness in human experience is the discrimination of qualitative differences. For cognitive reactions, for building up knowledge, apprehension of quality may be basic, and attention focuses upon it. Yet there may be other reactions in which it is not the quality but the attribute of intensity that takes first place on the stage. This may be the case in emotional reactions of the more primitive sort, where the intensity of stimulation becomes the functionally important factor, not the quality. For aesthetic reactions degree of intensity often becomes as important and often more important than sensory quality. There are individuals for whom a bit of music is carried almost entirely in terms of intensity changes plus rhythm. Primitive music, characterized as it is by a poverty of tonal variation and making up for this monotony by accent and rhythm, may present favorable material for a study of this question. Of two tones struck in rapid succession, the one softly, the other accented, the hearer is immediately aware very often of differences in intensities, but is in a quandary as to the relative position of the two experiences

in the tonal scale. It may well be that for his "immediate experience" the elements that came to consciousness may have had the marks of being auditory rather than visual experiences, but they may have been quite innocent of any specific quality mark; if *qualé* there was, it did not possess the same degree of clearness as did the intensity character. Apply an icy cold point never so lightly to the skin, and remove it quickly; the subject reports intensity of sensation, but often is in doubt as to the *qualé*. We may therefore ask whether the tendency to regard the quality as the "body" of the sensory experience, as over against other attributes, especially as over against intensity, is not a function, in part at least, of the habit of attending to the quality of the sensory content, rather than to its other "aspects". If this should prove to be the case, quality would be robbed of its unique position among the attributes.

Concerning the relation of the various attributes that are usually named as the inseparable constituents of a sensation element, we have thus far come to note the following considerations: We ought to observe some caution lest we put into the conception of "inseparability of the attributes" a meaning that is not yet warranted by experimental evidence. To affirm the inseparability of the attributes means only that they come into and go out of consciousness together. It does not necessarily mean that while they are in consciousness they are held together in a unity by some tie inherent in their peculiar psychological constitution, for it may well be that the readiness with which we attribute the "attributes" to one and the same "sensation", may be due to the operation of factors extrinsic to the particular "sensation". . . . We noted further that there is a tendency to regard quality as the more basic of the attributes, and the question presented itself whether this had to be accepted as an ultimate fact, or whether there are not conditions under which degree of intensity may come to assume the "body" rôle. If any introspective evidence should be brought to light that would show that this may at times be the case, then quality would be robbed of its seemingly unique position among the attributes. That

intensity may become the functionally important factor cannot be doubted on introspective grounds; that in some of the lower forms it is functionally *the* important factor, as quality for the most part is with us, seems highly probable; it therefore remains as yet an open question whether we ought unreservedly and without qualification ascribe to *qualé* the unique position of being the fundamental attribute, the "body" of the sensory experience. . . . And we saw in the third place that while there appears to be a close connection between the various attributes so far as their entrance into, and exit from, consciousness is concerned, it was nevertheless possible in the case of intensity and quality that they might differ widely with respect to their attentional clearness. This is in a certain sense a restatement of the point just made, viz.: that the various attributes might differ from time to time in their functional value. Nevertheless the restatement in terms of attentional clearness might prove to possess some significance.

XI

It is this last point that is of paramount interest for us: the status of clearness as an attribute of sensation, such as Titchener makes it, as over against Stumpf, who makes it an attribute of another psychological category: the *Funktion*. If it be true that under certain conditions attention may focus upon some one of the attributes of a sensation to the neglect of the others, so that it thereby attains to a greater degree of attentional clearness than do the others, then we would have an anomalous case of one and the same sensation characterized by two "clearnesses", and these not attributable to the sensation as a whole, but to various ones of its attributes. How this situation can be escaped is hard to see, for we certainly do have conscious experiences in the course of the serious business of life, in which certain ones of the aspects of sensory experience must be abstracted from, and attention given only to one, say quality or intensity. Or would

one claim that the process of attention in this case functions quite differently than in the case where we are attentive to other objects of normal experience? In a case of sudden need of a tool, say for prying open, we cast about and grasp the first article that appears to answer our purposes. The conventional use of the implement may be quite other than the service into which we are now impressing it, but in the present situation attention abstracts from all uses but its adequacy in the immediate situation. It is this aspect that is in the foreground of consciousness, and the meaning aspect of the implement in its normal function, if present at all, is certainly not so clear. Is the case essentially different when we come to the aspects of experience as sensory? We see that we are here in the midst of the same difficulty that we met before in the discussion of analysis. Indeed, it is nothing less than the very heart of that problem. Introspection, we are told, is the process of attending to the flow of conscious processes as conscious processes, with a view to noting parts and relationships between parts. A part is noted in that it stands out more clearly from the rest of consciousness, i.e., the part possesses greater attentional clearness. This part is then further scrutinized and it in turn divulges some simpler form of content. Finally we arrive at an element, say a sensation. We saw that while the psychologist starts out by saying that the sensation is the sum of all its attributes, he practically makes one of them, quality, the essence of the sensation, so that the other "aspects" become attributes of this one. We saw that under ordinary conditions quality is the aspect of sensory experience that is functionally most significant, and that we have therefore come to give it first place among the various aspects of sensation. In other words, quality is attended to and hence whatever attentional clearness may accrue to sensory experience under ordinary conditions, is in reality a clearness of quality. Whatever other aspects the sensation may present, they are in the background of consciousness. Let us examine for a moment the real import of one of the "conditions" of clearness. The most obvious of these conditions, says Titchener, is the intensity of the stimulus.[89] If

we introspect any of these cases in which an intense stimulus commands our attention, we find that the most pronounced character of the sensory content as it is precipitated into consciousness, is just its intensiveness. We are aware first of all that an *intense* stimulus has been presented and only after the first shock is over and the reflex adjustment of the sense-organ is complete, do we attend more specifically to the quality for purposes of further adjustment to the situation. We believe that it is only by a playing with terminology that one can escape the statement of the fact that the shift that takes place in the diverting of attention from intensity to quality is nothing other than a shift in relative clearnes. Titchener writes as follows: "When we are thus attending to extension or duration we may have very hazy ideas indeed about intensity and quality; precisely as, when we are observing intensity, we may have very hazy ideas about quality and duration."[84] There would therefore appear to be discernible differences of clearness in the various aspects or "attributes" of one and the same sensory experience, so that the status of clearness as an attribute of sensation is anomalous in this respect, for the doctrine of mental elements could not be reconciled with a visual sensation that was characterized at one and the same time by two hues, or two tints, or two intensities.

XII

But if it be true that within that sum total of attributes that, we are told, constitute the sensation, there may exist different degrees of clearness as regards the value of the various attributes for the attention process,—if this be true, still another difficulty would have to be faced in regard to the status of clearness as an attribute of sensation. The difficulty in question relates to the method of introspection. How do we ascertain the fact of clearness of sensory content? It was averred a while ago that we might attend to some one of the attributes of sensory experience whilst the other aspects receded into the background of the

field. Is this true also in the case of the attribute of clearness? Can we, for instance, become clearly aware that a given sensory content possesses a low degree of clearness? And if so, is the process by which we thus become aware of the attribute of clearness in any sense similar to that by which the other attributes of a sensory experience come into the foreground of consciousness? We would not raise a host of metaphysical queries in an attempt to give historical dignity and a backing of legitimacy to our question: we will therefore consider it on purely psychological grounds. The method of psychology, as of physcal science, says Titchener, is observation. Yet to distinguish it from the latter, we call it introspection, whereas the observation of physical phenomena is better referred to as inspection. Still there is an essential likeness between the two methods. In matching color on the color-wheel, in determining the number of tones in a chord that is being struck, there is practically no difference between introspection and inspection. "You are using the same method that you would use in counting the swings of a pendulum, or taking readings from a galvanometer scale, in the physical laboratory. There is a difference in subject-matter: the colors and the tones are dependent, not independent experiences: but the method is essentially the same."[72] When we come to consider more complex mental experience, however, it would appear at first blush as if the parallel between the methods of physics and psychology could not be maintained—as in analysing the mental reaction called out by the presentation of some word-stimulus, or in an attempt to observe a feeling or an emotion. "If you try to report the changes in consciousness, while these changes are in progress, you interfere with consciousness; your translation of the mental processes into words introduces new factors into that experience itself;"[85] and furthermore "cool consideration of an emotion is fatal to its very existence; your anger disappears, your disappointment evaporates, as you examine it."[87] Direct observation of the process while it is going on in consciousness, is therefore difficult and one solution of the difficulty is to observe it retrospectively, by making a *post mortem* examination of it, as it were.

But even in cases such as these it is not always absolutely necessary to employ the retrospective method, for "(a) the observations in question may be repeated", and thus stage after stage of the emotive process may be made the object of analysis; and "moreover, (b) the practiced observer gets into an introspective habit, has the introspective attitude ingrained in his system; so that it is possible for him, not only to take mental notes while the observation is in progress, without interfering with consicousness, but even to jot down written notes, as the histologist does while his eye is still held to the ocular of the microscope".[73] So much for the method of psychology according to Titchener.

Now let us examine the process by which we become aware of clearness as an attribute of sensory content. We saw how in the course of everyday experience either quality or intensity of sensation might become functionally important and attended to. Thus in the case of an intense stimulus suddenly breaking in upon us, we are aware at the first moment primarily of its intensiveness, and it is only after the first shock is over that we turn our attention exclusively upon its quality in our endeavor to adjust ourselves to this new factor that has been precipitated into the situation of the moment. If our purpose at the moment were the gathering of psychological data, we would have, then and there, the material for it. But our purpose happens to be to get ahead with the day's business and we therefore deal accordingly with the object that had served as an adequate stimulus for that mental experience in which clearness attached first to the intensity and then to the qualitative aspect of a certain sensory content. In attending to the quality of the stimulus for the purpose of discovering what it may mean, I am in truth using the method of inspection. I can continue to regard its quality and get it over and over again in a series of perceptual pulses, subject, of course, always to the limitations imposed by fatigue and by the "fluctuation of attention". So too, I could dwell upon its intensity, if necessary. One by one, these various aspects might be made to loom up in the foreground of consciousness. Transferred into the laboratory, the experimenter can introduce qualitative changes into the

stimulus and I would report change in sensory content *pari passu* as it occurs. I might be clearly aware that the sensation as it exists in consciousness possesses a very low intensity. So too, one might say, duration, and, if one chose, presumably also, extensity of sensation might become the object of our introspections in a way that would be essentially like inspection. But how is it with the attribute of clearness? Can we focus attention upon it as we can in the case of the other attributes? Here a difficulty presents itself. Introspection as regards clearness would seem to be impossible in the sense of inspection. The moment we would dwell upon the clearness aspect the content comes into the focus of the field of attention, i.e., there is a change in the aspect concerning which we would introspect. Thus it has come to pass that most of that body of knowledge that the psychologist has gathered concerning sensation has been obtained under conditions of maximal clearness; and it is one of the chief counts against the attempts to make the unanalysed content equivalent to the analysed content, for it must yet be shown whether analytic attention does not perhaps effect other changes in the sensory content besides that of clearness. But as regards clearness itself, a low degree of clearness can under normal conditions be determined only retrospectively. While I can attend without difficulty one by one to the qualities that are presented to me for purposes of introspection, until I have reported upon the entire series; and while I can attend one by one to the whole gamut of possible intensities that can be experimentally induced, and note introspectively in the sense of inspection my conscious reaction upon each one of them while it exists in consciousness;—while an observer can do these things in the case of quality and intensity, the situation is otherwise when we come to clearness as an attribute of sensation. The ordinary observer, at least, will have great difficulty in noting introspectivly varying degrees of clearness without thereby bringing the content to the focus of attention, i.e., making it maximally clear. Introspection, in the sense of inspection, calm regarding of the content without thereby changing it with respect to that aspect of it with which we are for the time being

primarily concerned, is impossible in the case of clearness. He cannot ordinarily note thus the gamut of possible clearnesses while they are still a part of conscious experience. Our difficulty in introspecting (in the sense of inspecting) a sensation in a state of low degree of clearness, is in a sense similar to that which we experience when we attempt to introspect a feeling while it is still in consciousness. The feeling vanishes when we would attend to it. The unclear sensation may not vanish, to be sure, upon our turning to it, but it is changed with respect to that condition concerning which we were seeking knowledge.

XIII

We would now ask: What are the implications of this difference in the status of one of our "attributes"? It appears that not every aspect of sensory experience can be attended to introspectively in the sense of inspection. If this be so, then it means that all that large section of our sensory experience that falls within the outer zones of the field of attention cannot be gotten at by direct introspection, but must be attacked by the same methods as are used in the case of emotion and other complex processes that are interfered with when directly attended to. In other words, if clearness be accounted an attribute of sensation, then there is one respect in which it may behave in a manner that is usually regarded as a differentia of emotional and of "complex" processes. Not that at this point we would infer from this that clearness must therefore be conceived as partaking essentially of the character of an attribute of an affective or a complex content. Only this: that it seemingly makes the status of clearness in a certain sense unique among the attributes of sensation, if we regard it as such. And furthermore it raises the question whether the noting of this fact may not have been operative as a motive in the minds of some psychologists in making clearness the attribute of some other category, either of content or of function or

activity. Thus Stumpf says, "Phenomena with their attributes are given us, stand over against us as somewhat objective, that possesses its own laws, a somewhat that we have merely to describe and acknowledge". (*Die Erscheinungen sind uns mit ihren Eigenschaften gegeben, stehen uns als etwas Objektives. Eigengesetzliches gegenüber, das wir nur zu beschreiben und anzuerkennen haben.*)[69] Now this means nothing other than that it is essentially of the nature of phenomena, among them sensation elements, to be present in our consciousness in such a way that they may be calmly inspected and noted while they are a part of our experience. They stand over against us endowed with a certain objectivity, governed by laws of their own. Clearness for Stumpf does not behave in this way, and it is significant that he does not make it an attribute of sensory content, but of the activity in which the sensory content is manipulated. It is the attribute of one of the *Funktionen,* viz., the activity of awareness, of apprehension.

Thus we are in a position to understand somewhat better Stumpf's doctrine concerning the relation of his phenomena and *Funktionen.* We become conscious of the *Funktionen,* in this specific case of the activity of awareness with its attribute of clearness, "by a direction of consciousness that is other than that in which we receive our knowledge of colors". He cites Locke, Leibnitz, Sigwart, Lotze, Brentano, Dilthey, Volkelt, Erdmann, and Lipps. All of these, says Stumpf, "believe that they are able to grasp the psychic life in its very weaving, whereas colors and tones are apprehended merely as contents of acts of awareness, i.e., these sensations are apprehended as contents in a special class of psychic activities".[52]

Let us turn now to a consideration of what may be meant by this "other direction of consciousness" by means of which the psychic activities are apprehended. The classic passage in Locke on this point reads as follows: "The mind, receiving the ideas mentioned in the foregoing chapters from without, when it turns its view inward upon itself, and observes its own actions about those ideas it has, takes from thence other ideas, which are as

capable to be objects of its contemplation as any of those it received from foreign things."[38] Some of the "modes of these simple ideas of reflection" are "remembrance, discerning, reasoning, judging". Let us compare this "turning of the view inward" with the method of observation mentioned by Titchener. He tells us that "the practiced observer has the introspective habit ingrained in his system", so that he can "take mental notes" and even "jot down written notes", while the observation is in progress, and that he can do this "without interfering with consciousness", just "as the histologist does while his eye is still held to the ocular of the microscope".[88] If the phrase "without interfering with consciousness" has any meaning here, it can mean only this: that at this point at least we could not count on Titchener's agreement with us as regards our analysis of the status of clearness, when we would attend to this aspect of some marginal experience, *qua* marginal; for we believe that it is inherent in the very nature of consciousness that the moment we would introspect, in the sense of inspect, that which is now marginal, our attempt to do so results in making it focal, and thus interferes with, changes, in a very real sense, the ongoing consciousness with respect to that very aspect of the activity that we would "inspect". But applying the phrase to our attempt to introspect such aspects of consciousness as the degree of certainty in judgment, or such an experience as denial or affirmation (*Funktionen* for Stumpf, for Titchener, very probably, "attitudes"), it can mean only this: that he can look inward upon the doubt and study it as to its psychological constitution without defeating the ends of the activity in progress, and what is more, without changing the quality of the conscious "feel" of the "attitude" in question. Stumpf, we believe, would take issue with him here. He believes that they may be "given" in a very real sense in consciousness, but "by another direction of consciousness". This we would interpret as meaning that while "doubt" as a conscious somewhat is just as truly present as is the sensory experience of coldness, it is doomed to remain forever peculiarly marginal. Let us turn now for a moment to Locke, whom Stumpf cites in

support of his position. It may be that we may get some further light there as to just what it is that constitutes this other class of psychical ultimates.

The account in Locke is too meagre to give us any basis for inferring whether or not he would say as Titchener does that we can watch the activity from the psychologist's point of view without interfering with the normal process,—but he does say that we may become conscious of the act aspect, that is, while we may not stop to formulate overtly psychological judgments concerning it, we nevertheless find that some feeling of activity, differing from time to time as the situation varies, colors our immediate experience in the course of our efforts to adapt ourselves in our world. Referring to different aspects of this activity consciousness, we may say that we have had a memory experience, an emotion, a volition, or what not. According to Locke, we might be conscious of these activities while attention was busied in the main with the materials with which we are dealing. But the activity aspect is not singled out by attention in the same way as is the perceptual content. . . . When Locke says that anyone can observe the actions of perception and volition within himself, he is without a doubt referring to something immediately experienced. These facts of immediate experience are the empirical data for the ideas: thinking, willing. While the concepts of "thinking" and "willing" may arise as a result of discursive thinking, they yet have a basis in immediate conscious experience, viz.: the "operations of the mind about its ideas, including the passions sometimes arising from them, such as the satisfaction or uneasiness arising from any thought". The "foreign things" give rise to "ideas of sensation". But besides the consciousness of such percepts and ideas, we are aware also of the "operations" of our mind.[37] Yet normally it would interfere with the business in hand if we were to turn to attend to these operations. To attend to the operations of the mind is not the normal pursuit of man, and these awarenesses of mental operations leads a penumbral existence off on the margin of the field in the form of vague "feels", meanings, or what not. Most often, probably, they

remain below the threshold without conscious existence. Yet at other times these awarenesses may become very vivid indeed, and if our interests have a psychological bent, these experiences yield material for our ideas of psychological categories. This elaboration into concepts is the work of "reflection" for Locke; for Titchener it probably occurs in the process "of taking mental notes while the observation is in progress, without interfering with consciousness". It is true, therefore, as Stumpf says, that Locke subscribes to the doctrine that we are conscious of, and observe in ourselves, the "actings of the mind", such as "perception, thinking, doubting, believing, reasoning, knowing, willing". "This source of ideas every man has wholly within hmself; and though it be not sense, as having nothing to do with external objects, yet it is very like, and might properly be called internal sense." For Locke, then, consciousness may be analysed into percepts, memories, and imaginations (in Locke's own terminology, all these would be comprehended under the term: ideas of sensation), on the one hand, and consciousness of mental "actings" (giving rise in "reflection" to ideas of reflection), on the other. In this sense it would appear that Stumpf is correct in claiming that Locke taught the possibility of an awareness of *Funktion* ("actings of the mind").

XIV

Thus Locke and Stumpf. Turning to Titchener, we have seen that for him the immediately given is the percept. It is this that the psychologist analyses into its elements. Titchener is clear on this point: that the sensation and image elements that he analyses out are not given as elements in the immediate experience of the subject, but come to consciousness united into a whole, the percept. His immediately given then is that totality that Locke called an "idea". It is what James has in mind when he speaks of "the objective fact, known to us as the peppermint taste". It is my immediate consciousness of the peppermint, before I note

that it is made up of certain sensations of temperature, smell, contact. Now as to Titchener's position as regards the other category, the category of psychic *Funktion*, of mental activity. He says, "there are, in a certain sense, a hearing, a feeling, a thinking, which are distinguishable from the tone and the pleasure and the thought".[93] If they are "distinguishable" aspects, one might ask why they are not "attributes" in the same sense as the other "attributes", even though they be not "ultimates" as Stumpf would make them? Logically, it would be somewhat inconsistent to leave them aside, whilst demanding that certain other "attributes" which were obtained by a very similar process of "distinguishing", of analysis, or what not, be attributed to some psychological ultimate. And this is practically what he does do when he says: "Only the distinction comes to me, not as that of act and content, but as that of temporal course and qualitative specificity of a single process. . . . The way in which a process runs its course, that is its 'act', that is what constitutes it sensing or feeling or thinking; the quality which is thus in passage, that is its 'content', that is what constitutes it tone or pleasure."[92] Act and content are for Titchener two ways of looking at the same "process". But we ask: Are not for him the singling out of "intensity" and "quality" also nothing more nor less than "two ways of looking at 'one and the same process' "? The "acting" is the "temporal course" of his particular types of psychological ultimates, probably their "durational" aspect. The question may become of fundamental interest, however, for certain psychologists, whether these two ways of looking at the same "process" are in any way a factor in the "process". But Titchener is satisfied to analyse the "single process" into its elementary processes: sensations, images, feelings, and to describe these with reference to their "attributes". He summarily dismisses the claim of the "act psychology" by calling it a psychology of reflection, a psychology in which "logical construction has forestalled introspective examination".[93] He believes that "we have in the idea of 'process' an instrument of analysis that is adequate to its task, and that it relieves us from the fatal necessity of asking help from logic".[94]

XV

Leaving aside Titchener's imputation that the act psychology is a substitution of logic for psychology, let us turn to Stumpf and note once more the distinction that he believes to be fundamental between his phenomena and his *Funktionen*. A psychologist such as Stumpf might say to Titchener: Having admitted the "givenness", the "distinguishableness", of the act aspect, would you assert that this aspect comes under the same category as the attributes of intensity and sensory quality? That the durational aspect, the "temporal course" is sufficient to account for the manifold variety seemingly occuring as regards this activity aspect? Stumpf would affirm you cannot. No other predicate of the activity aspect excepting it be that of duration, can be attributed to the phenomenal aspect. There is a degree of clearness of apprehension, degree of certainty of judgment, etc. (c.f. Section IV above). As regards the second, Stumpf maintains by definition, that these aspects of activity, cannot be attributed to the some category as the other attributes of sensory experience. And his distinction is made on the basis of difference in behavior.

Stumpf says that his phenomena with their attributes stand over against us as somewhat objective that we have merely to describe and acknowledge. These phenomena, then, as thus defined, would, it seems to us, come unequivocally under the head of those aspects of conscious experience which could be examined introspectively in a manner which would be essentially the same as inspection as it is employed in the physical sciences, as over against retrospection. Here then is a point of departure from which we can start out and examine the status of several fundamental categories in the two psychological systems.

Titchener and Stumpf would agree in this, we believe; that the sensory and imaginal material that constitutes the bedrock of the perceptual and ideational experiences—that this sensory experience is capable of fixation in such a way that it may be "inspected" (excepting of course the attribute of clearness in the case of Titchener's "sensation elements", which Stumpf would

make an attribute of *Funktion*), in such a way that it may be calmly regarded, observations made concerning its duration, quality, intensity and any changes that occur in these aspects from time to time. Stumpf's statement concerning his phenomena would indicate agreement on this point.

On the other hand we believe that if we were to take the list of categories of *Funktionen,* of mental acts in Stumpf's sense, we should find that the attributes in terms of which they are according to him given to us, will show that these, as aspects of consciousness, must be subsumed under the second heading on the basis of behavior when attended to introspectively. They are all aspects that change or evanesce when attention would focus upon them as psychical. "Cool consideration of an emotion is fatal to its very existence; your anger disappears, your disappointment evaporates as you examine it," says Titchener.[86] Try to fixate the willing, judging, comparing, and the other intellectual acts, in so far as they throw any reflection into consciousness over and above the object willed, the idea judged, the contents, whether perceptual or ideational, that are compared,—try to fixate these, and the activity is balked. The degree of clearness, according to Stumpf the attribute of the *Funktion* of awareness or apprehension, cannot, we saw, be directly attended to for purposes of psychological study. The same thing holds of doubt. Try to fix attention upon the doubt as a mental somewhat and the "doubt" consciousness is replaced by something other that may be sensation or what not, but it no longer is that consciousness which everyone, when called upon, refers to as doubt. So too, the consciousness of certainty in judgment. So too in the case of that "plus" which gives to an experience memory tang rather than a perceptual coloring, or that which constitutes negation rather than affirmation—so too, in recognition desiring, etc. In all these there is a very actual consciousness that is not focal, but marginal, and that eludes us when we try to introspect it directly, in the sense of inspection, in the sense in which we seem to inspect the "object" with which we are dealing during these experiences, i.e. in the percept, the memory image, the matter-of-

fact assented to or dissented from. It is here that the psychologist appears to be under the unfortunate curse that the moment he would lay his hands directly on one of these states of consciousness it turns to dust and ashes at his touch. Try to catch the doubt, as it passes through the living consciousness, and you destroy it and come away with shreds of sensation located in muscle of head and eyelid and mouth. Try to introspect the consciousness of certainty and what just now was a very vital aspect of immediate experience turns to nothingness. You can experience it, but you cannot inspect it. And it seems it is therefore that Stumpf says that "it is by another direction of consciousness" that we become aware of the *Funktionen.* While one might have welcomed greater explicitness on this point, we do not believe that we are guilty of misinterpretation when we infer that this statement has reference to the method by which they may become objects of psychological knowledge for us. We may infer that they are intrinsically so constituted, that they may not be apprehended introspectively in the same way as are the phenomena. We are further convinced that this is the real ground for Stumpf's distinction between phenomena and *Funktionen* by the fact that he makes clearness an attribute not of phenomena but of *Funktion*—and we have already seen that clearness as an aspect of conscious experience seemingly behaved differently from the usually recognized sensation attributes when we purposed to gather introspective data concerning it. For these reasons we believe that we are justified in the interpretation just given and we shall proceed to a study of the legitimacy and the implications of a classification of psychological processes on the basis of the manner in which they behave when an attempt is made to observe them. Applying this provisional criterion to Stumpf's categories, it would appear that his phenomena would come under the head of those conscious processes that can be examined introspectively in a manner which would be essentially the same as "inspection," and our justification in proceeding thus lies in the character of Stumpf's definition which is couched essentially in terms of the behavior alluded to above.

As regards procedure, therefore, we cannot agree with Titchener that the method of psychology is essentially the same as that of the physical sciences, viz., that of immediate inspection of the material that constitutes the objects of the science; for it appears that some aspects or complexes of consciousness normally cannot be directly inspected without interfering with that aspect with reference to which we are seeking psychological data; it may be, perhaps, that the methods can be the same when we are dealing with certain aspects of consciousness, but it must be essentially different when we are dealing with these others that are "transient, elusive, slippery," those that "refuse to be observed while they are in passage." So long as "cool consideration of an emotion is fatal to its very existence," so long as "your anger disappears, your disappointment evaporates, as you examine it," so long as we must thus qualify the statement that the psychologist observes his objects in the same way as the physicist does his, just so long must we recognize a difference in behavior on the part of the mental processes that we are studying, a difference in behavior that necessitates a different mode of attack on the part of the psychologist; and we must be careful to refrain from stating propositions that may in any way be taken as general when in reality they are particular or qualified, such as "introspection is very like inspection," "in general, the method of psychology is much the some as the method of physics," "the method of the physical and the psychological sciences is substantially the same."[74]

XVI

Following this line of immanent criticism of our psychological systems, we have come to the distinction between phases of conscious experience that are amenable to immediate survey, and other phases that under normal conditions elude inspection for psychological purposes, that cannot be focused upon without changing or destroying them. By means of this distinction let

us examine the several psychological categories and let us note just what happens when the conscious experiences designated by them are attended to. There is the *Funktion* of Stumpf, the "attitude" of Titchener, the meaningful percept and idea—all to be examined in this manner; and finally the "sensation" of current psychology, which seemingly has in common with Stumpf's phenomena that it may be examined, attended to, "inspected," without changing it or interfering with it as a conscious "process."

XVII

Referring to the experience of doubt which Locke and Stumpf would designate as an "acting of the mind," as a *Funktion,* we saw that it may be definitely an aspect of conscious experience, yet when attention turns from that which is doubted and tries to catch the doubt itself, it will surprise not the doubt, but perhaps certain muscular sensations mediated by contractions of the facial muscles. There are to be found nowhere in the literature, since James' inimitable chapter on the will, finer and keener introspective accounts of just such "catchings" of these fleeting mental states, of "attitudes", than those recently given by Titchener in his survey of the "experimental psychology of thinking".[96] On the side of investigations of muscular expression belonging to various forms of intellectual activity, there is excellent material collected by Sancto de Sanctis in his "Mimicry of Thinking". But the objection is unanswerable that these kinaesthetic sensations that attention lights upon when it pounces upon such a subtle psychosis as a state of doubt, are not the psychic equivalents of the state itself. As Titchener says concerning emotion: "a group of organic sensations is, after all, a group of organic sensations; palpitation of the heart, is not, in itself, the emotion of dread, and blushing is not, in itself, the emotion of shame."[81b] Concerning recognition, again, which is essentially of the nature of *Funktion,* Angell says; "In all instances of con-

scious recognition however, it must be remembered that the *mental act* of explicit recognition is something unique; something which is not simply synonymous with the accompanying conditions which we have been describing."[1] In the case of emotion (which the *Funktion-psychologen* rank among the psychic activities) we find the problem already attacked. A theory such as the James-Lange theory of the emotions avers that in this total state the emotional tang is given, in a large measure at least, by the sensory back-stroke arising from certain organic and muscular reactions that have been reflexly set up in the organism. The facts at the basis of this theory are ascertainable by means of the "pouncing" above described and by the method of objective observation. The "pouncing" reveals certain organic and kinaesthetic experiences; objective observation of the person during the emotional state itself shows certain objective movements, and apparatus properly applied would reveal certain circulatory and respiratory changes which tally with the subject's introspective account of what was present at the time when the emotional aspect of the total experience was "pounced" upon. Now in using these data in the explanation of the emotional psychosis, the psychologist does not affirm that the consciousness of the analyzing experience is the same as the emotional phase itself; but he does believe that he has grounds for assuming that the sensory stimulation that functions in mediating the "sensations" precipitated by the analysis, was operative in the original state and lent color to it. The modern psychologist is too conscious of the "Jabberwock of the psychologist's fallacy" to be guilty of committing it at a point where it is so easily detectable as here. He realizes that the emotion is not the equivalent of sensation *a* plus sensation *b* plus certain affective elements, etc., that are the products of analysis,—but that the emotion was experienced differently, not under conditions of analytic attention ready to note any organic or kinaesthetic sensations that might arise, but as a unified conscious reaction upon a stimulus that is an adequate provocative of that particular form of racial response.

The method of investigation of the consciousness of intellectual

Funktionen and of "attitudes" must be essentially the same as that in the case of emotion. The introspective data gathered after the manner described by Titchener, the objective expressive signs accompanying the different types of concentration and intellectual process, as given by deSanctis, introspective observation (in the sense of retrospection) as to how far the changes in the pattern of the percepts and ideas, that are at the focus of attention, may contribute to this consciousness of mental activity,—these will constitute an analytic account of the activity consciousness. And while it is thus analytic, such an account will not fail to do justice to the fact that the "feel" of the activity consciousness as a real vital experience appeared to consist in a somewhat quite other than merely the consciousness of the sensory factors just described. Indeed, such an account will recognize that while it is necessary to attribute some function to kinaesthetic stimulation *and to changes in the content at the focus of attention,* in bringing about the consciousness of psychic activity—this function will yet be different in the original experience than under conditions of analytic attention. One difference in behavior of the sensory factor is this, that just as in emotional experience, it is not at the focus but at the margin of field of attention. All considerations that were urged a while ago on this point must be taken into account. . . . To determine these differences in the sensory content under different conditions of clearness, i.e., of focal and marginal apprehension, is one of the problems that is just now confronting psychology. Another difference is this: that in the original experience these various stimuli functioned in evoking an unanalysed unified response of consciousness. And, we believe, closely related to this, is the third difference. The difference in the "feel" of these stimuli in the original and in the analytic experience. In the original experience this sensory stimulation gave rise to a different "feel" from that which arises in analytic attention: there it signified doubting, affirming, negating, desiring, rejecting, attending, purposing, analysing, believing, discriminating; here it signifies so many sensory-"elements"-precipitated-in-pursuance-of-our-

attempt-to-discover,-to-know,-what-doubting-affirming,-purposing -really-is. It is this "meaning" or "feel" aspect, we believe, that Stumpf hypostatizes as his awareness of psychic *Funktion* which for him is "immediately given" in the same sense in which his "phenomena" are immediately given.

We trust that we will not be misunderstood in designating the consciousness of *Funktion,* of act, a meaning. It will be better perhaps to leave this term to express the analogous aspect of percepts, ideas and concepts, and to speak, in the case of the *Funktionen,* of the "feel" of the act, without of course, implying necessarily the presence of affective factors in this "consciousness of act". But the problem is for us essentially the same in the case of the "feel" of the mental act, and the "meaning" of a percept or idea. It suffices here to note that in the case of the consciousness of psychic *Funktionen* and of attitudes, when such arises within the total consciousness, it is the analogue of the meaning aspect in the case of percept or idea consciousness. It is this that is changed, that evanesces, that will not suffer immediate fixation.

XVIII

Let us turn now to an examination of the percept and idea at the hand of our provisional criterion of behavior under conditions of immediate fixation. I glance over a page before me and a word catches my eye. The consciousness of the word is a percept. There is a presentation and it comes to me meaningfully. If I continue to dwell upon its meaning, this appears to remain the same, only it becomes what we are pleased to call: more explicit. Images arise in consciousness organically connected with the meaning. The totality is the percept. Analytically there is the sensory stimulus, the visual impression of the letters on the page. Added to this there may be in consciousness an auditory image of the word, and kinaesthetic images, or even sensations, of its enunciation. But this is not all, there is the meaning. The visual impression, the auditory image, the kinaesthesia;—these

are not the meaning—they "call up" the meaning. Dwelling upon this meaning aspect results in making "explicit" what before, at the first glance at the word, appeared "implicit"; and the process of becoming explicit is the unfolding of a new perceptual and ideational complex, germane to the initial ones, yet not the same. This new complex may again be analysed into sensory or image elements with a halo or fringe of meaning. Try to fixate this halo or fringe and once more it becomes explicit in the form of another idea and so our attempt to fixate the meaning as a psychic content leads us ever on to other though related ideas. The attempt to introspect the meaning aspect of percepts and ideas, then, is impossible in the sense of immediate inspection. The point we would stress here is only this: that the attempt to "catch" the meaning results in the coming into consciousness of a new ideational complex with its new meaning—or else you find yourself contemplating the objective stimulus, *qua* stimulus, that had given rise to the original percept—or else you "come to" from out of a period of seeming nothingness.[3]

XIX

But let us see what happens when we attempt the analysis by concentrating attention not on the meaning aspect of the percept, but upon the stimulus aspect. We must remember that in the ordinary perceptual experience there is no differentiation of these two aspects, and it is only because in problematic situations the two have become differentiated, that we assume that every actual percept of ordinary experience has these two aspects. The experience in which the distinction first comes to consciousness may be described as follows: Say that we are looking at a word on a page and fix attention upon its visual form, upon its sound, upon our enunciation of it. It carries with it its accustomed "meaning," but presto! we suddenly say: "Curious—that this collection of visual things, this sound, these enunciatory movements, should mean *that*." If we continue to look at the word or contemplate

its sound, or the feel of it in our throats, its meaning becomes more and more effectually estranged from it. Attention is busied with noting parts and relations in the sensory complex, and this thing that for us was ever so familiar now becomes a monster of strangeness. New associations flash through our minds that are a result of our attention simply to the form of the word.

It is in experiences such as these that the psychological distinction between stimulus and its meaning has its rise. For many persons, children especially, this process of divorcing the auditory, visual and kinaesthetic stuff, both sensory and imaginal, from the "meaning" of a word possesses a great fascination. This process, this act of separating, may therefore become a veritable habit and can be operative not only in the case of words, but also in the case of the perception of the common objects of daily life. It is this that the modern mystics have made the central feature of their method of reaction upon the experience that the life of our day yields them. It is thus that they re-introduce into their world the sense of mystery that for the common run of men is fast becoming an unknown thing. The extent of scientific control of our objective world has become so great that small margin is left for arousing reactions of wonder, awe and mystery. Yet by the apprehension of the significance of the psychological process just outlined, the modern man and woman of the "mystic temper" has found the way that leads back into the world of mystery, a world more wonderful by far than any wherein dwelt their predecessors of early days, for the modern mystic enters the world of beauty not through a narrow range of experiences as did the mystic of old; no, every experience of the common life is to-day the gateway that leads thither. . . . But we shall return anon to this relation between "stimulus" and "meaning."

The citation of cases of the genesis of the distinction between stimulus and meaning is not made for the purpose of leading up to a discussion of what meaning is, but rather for the purpose of noting what happens when attention tries to analyze the percept by focusing upon the presentation aspect. We saw that focusing

attention upon the meaning aspect resulted not in new light on what meaning "really" was, but tended to bring into consciousness other though related ideational content. We see now that attention to the stimulus aspect likewise tends to disintegrate the original percept, divorcing the stimulus from the original meaning and bringing it, the stimulus, to consciousness in a new way; i.e., it acquires new meanings that arise in consciousness with the new pattern into which the stimulus falls under our continued inspection. And if our interests are psychological, the stimulation will take on the meaning of "sensations" and "images" that have certain "attributes" with reference to which we may examine them. The old meaning has fled and instead there is the new one that accrues to the stimulus through the new situation and its dominant purpose of psychological analysis. The meaning is gone, but the "sensations" remain. We see therefore once more that now when the distinction between stimulus and meaning has already become ours and we attempt to get at the percept by fixating attention upon the stimulus aspect, the original experience is changed in the process, the original meaning evanesces and we find that analytic attention is contemplating a congeries of "sensations," or in the case of the idea, an "image"-complex.

So our attempt to "inspect" a percept or an idea results in the precipitation of "sensations" in a process analogous to the one in which we erstwhile attempted to fixate an aspect of consciousness giving us the "feel" of some mental "activity", an awareness of hesitation, of doubt, of concentration, or what not. There we saw that the "feel" evanesced and that attention was focused either upon "organic attitude and its kinaesthetic representation,"[96] or else there was an immediate memory of that phase of the perceptual and ideational process that was going on in the original experience at the moment when the "feel" of activity was present. So, too, here, in the case of our attempt to fixate the percept we find that it is impossible to introspect in the sense of "inspection" without interfering with the conscious activity that is going on. The "meaning" evanesces and we find that attention is busied with the contemplation of the "sensations" that

are mediated by the stimulus, and of their configuration and "attributes."

XX

Here, however, in our search we have come upon something in the way of consciousness that seemingly can be examined, in the words of Titchener, by a method that is substantially the same as the method of the physical sciences. In the words of Stumpf we are now dealing with sensations, one of the classes of phenomena that "stand over against us as somewhat objective, that possesses its own laws, a somewhat that we have merely to describe and acknowledge." And so for the third time we return to a consideration of that factor in consciousness that, above all others, appears to many of us as most palpable, most substantial and least elusive. Yet its palpability, substantiality, relative stability, may be only seeming; though compared with other aspects of conscious life, it appears like a great rock into whose shadow we may ever return, from our wearisome chase after the psychical. But πάντα ῥεῖ and "the eternal hills are little by little breaking up and wearing away," and in that "great rock" changes, too, may be relentlessly going on. Figures aside—let us examine likewise the behavior of the sensory aspect under conditions of introspective attention with a view to discovering in what sense, if any, it may be said that it can be "inspected" without introducing changes into it, without interfering with it.

Let us begin by taking Titchener's example illustrative of what is for him unambiguously a case of sensory experience. "Take one of the familiar puzzle pictures, a picture which represents, we will say, a house and a garden, and somewhere in which there is concealed the outline of a human face. As you search for the face, the contents of the whole picture are at the conscious focus. Suddenly you find it: and what happens? Why, as you do so, the picture drops clean away from the focus; the face stands out with all imaginable clearness, and the house and garden are

no clearer than the feel of the paper between your fingers. The experience is very striking, as I have described it: it is more striking still, if the face baffles you, and you go off on false scents. For every time that you think that you have found the hidden outline, the picture slips from you,—slips, to come back with a mental jerk as you realize your failure." The "mental jerk" is exquisitely descriptive of the way in which a new meaning attaches to a stimulus. For Titchener, to be sure, it is a change of clearness in the sensory content, yet the fact that he describes is patent enough. What before was, at most, vague, is now made clear, and its clearness comes in with a mental jerk. Now it is in this moment of mental jerk that we become aware of the fact that the previous awareness of this content was far vaguer than just at this point of entrance of the interpretation in which the part-content is unified and brought to the crest of the wave of sensory attention. The point to note is this: that the sudden coming to the crest on the part of the part-content may become the occasion of the psychological judgment that the sensory content which is the basis of the new percept is now clearer by far than just a moment ago. And now, having found the face in the picture, the psychologist might ask, does the face, or rather, does the sensory experience that constitutes the percept "face", do these sensations continue at the initial level of clearness or not? Since Titchener has not carried the psychological description further, we shall attempt it ourselves. Having found the face, we may do one of two things: dismiss the whole matter and turn to the business of life, serious or otherwise; or we may continue for a while longer to attend to the face. Again, if we do the latter, we shall discover later, upon retrospection, that one of three things has happened. 1. We suddenly found that the sensations meaning "face" have wandered off, and instead we are thinking of other things; further retrospection may discover to us, perhaps, the associative nexus that lead from "face" to the present ideational content. 2. Or we note that attention continued to focus upon the sensory material before us and specifically upon that part of the content that was welded together in the face-

percept, but that we did not rest content with the initial total face-meaning, and went on to attend to nose, eyes, ears, mouth, etc. If in so doing we saw something unusual in the conformation of any of these parts, say in the shape of the nose, we note, retrospectively once more, that such apprehensions seem to come home to us with another mental jerk, slighter perhaps than the original one, yet nevertheless with more clearness than attaches to one which is representative of an accustomed type. 3. Or in the third place we may find retrospectively that we have done neither of these things after choosing to busy ourselves further with the face, rather than to return to the duties of the day. We may have looked again, and then again, at this face, trying perhaps to get once more some subtle general impression that was imbedded in our original perceptual reaction upon the part-content. We continue to look at the face and succeed in getting the impression sought,—and then we find that we have gone off into a doze, or have even fallen asleep, and are aroused only by some sudden stimulus, more or less intense, external or internal. The many methods of putting one's self to sleep by "watching the sheep jump over the stile", or "looking at the tip of one's nose",—all have this element in common, and their effectiveness in many instances cannot be questioned.

We note this third type of general behavior also in the case of another actual experience, viz.: in one of the methods used to induce hypnotic sleep. A bright metallic ball may be placed before us and we are told to center our whole attention upon it. Pursuant to this command we focus upon it. We are to "think" of nothing else, but just fixate the ball. Very well, we note the form, then its size,—just try to "apprehend" them. If attention would go off on the track of associated ideas, we nevertheless feel a "pull" back to the ball. The distribution of light and shade upon its surface "strikes" us. Then we note the color, etc., but soon the possible ways of apprehending the ball without calling up other objects is exhausted and so once more we go through the various ways of apprehension just mentioned. While the perceptions were thus being repeated over and over, conscious-

ness as a whole sinks ever to a lower level, and unless in some way the total situation becomes operative by arousing in us some strong impulsive reaction such as is involved in the thought of the "unworthiness" of this our conduct in which we are giving ourselves over into the control of another—unless something of this sort happens, we soon are in an hypnotic sleep.

Again, in the case of ambiguous drawings, when one meaning is "in", and the other is "out", the two alternate rapidly enough, but never is the stimulus apprehended in both ways at once. It is another question how it is that we may "know" that the other meaning is possible. And it is beside our purpose to touch upon that here, except to note the fact that in the complex consciousness, it would appear that at times we do have some sort of awareness of the potentiality of this other perception. It may be that this is analogous to our awareness of the whole to which a part belongs, but this problem will occupy us elsewhere. Here let us note only that the sensory stimulation tends to release alternately two perceptual reactions. In these cases in which the sensory stimulation attended to is coterminus in the two interpretations, the same "mental jerk" is experienced as in the case of the puzzle picture in which a part was abstracted in the perception of the hidden face. Since the sensory field that is involved in the area of maximal clearness remains the same in the case of the ambiguous figure, we can there interpret the facts only by saying that that which with interpretation A had been made clear, had in the meantime become vague and is once more brought to maximal clearness with the coming in of interpretation B.

Now a "sensation" too, behaves as do those supposedly "complex" objects of perception. In our attempt to "inspect" it, Titchener tells us, we may perceive it now with this "attribute" in the foreground of attention and now that.[84] Whenever any aspect of the sensory stimulation arouses consciousness to a maximal degree, it was always a case of a consciousness of that particular aspect of the stimulation coming as its meaning. The stimulation that finds no reflex pathway over which to discharge

in terms of established reflexes calls out the attention reflex as its response and this phase of the impeded activity is precipitated into consciousness as the total situation: "what's that?" and thus opens the pathways for possible discharge by way of the cortex. This coming to clearness in consciousness on the part of the various aspects of the "sensation" is in every way like the "mental jerk" by which Titchener describes the coming of a new percept. In the period just following the jerk there is no awareness of a subsidence of the clearness of the "sensation", but such a decrease is inferred from the fact that in the new pulse of perceptual reaction, the content once more becomes maximally clear. The attempt to keep the sensation in consciousness, as in the case of the metal ball experience, results eventually in a lowering of the degree of clearness of consciousness to the threshold of sleep.

Since the attributes behave thus in every way as do the meanings of "complex" percepts, coming now one to the focus of consciousness and now another, there is some ground to doubt the claim of "sensation" that must have all its attributes in consciousness or none,—to the place of a psychological ultimate. For if in the case of ambiguous drawings it is not necessary for the two possible meanings to be in consciousness at the same time as "percepts",—if indeed it is not only not necessary but in the nature of the case impossible to have the two present as "percepts" at one and the same time—but that one of them, if it be represented in consciousness at all can be present as "idea" merely, then it must be shown why the various "attributes", which otherwise behave in every way as do the several meanings of ambiguous objects, should form an exception in this one respect: their inseparable presence in consciousness.

If it be true that we can single out some one "attribute" of sensation, say the brightness of a visual experience, and focus upon it, and that "we may have very hazy ideas about quality and duration"[84] as when a bright light suddenly flashed before us on a dark night,—if in that case brightness be the attribute focused upon, and our "ideas" of the other attributes may be very, very vague, how are we to determine the point of vagueness beyond

which they may not go, unless they would drag the whole sensation down below the threshold? The more restricted attention is, at this critical moment of our experience, upon the intensity phase, the nearer the danger of its forcing the other aspects below the threshold of consciousness—and presto! the whole light sensation would have to disappear, if it were true that the "nullifying of any attribute annihilates the sensation."[84] But it is far from our purpose to caricature a venerable concept. We are voicing only a growing conviction that the concept is in need of reconstruction and reinterpretation.

We find that the "attributes" of "sensation" behave just as do "meanings" in the case of ambiguous drawings. The different aspects come to consciousness with the same "mental jerk"; when one "attribute" is in the foreground the others recede, just as two interpretations of a stimulus in the case of the drawings; and like the attempt to keep some particular conscious state in which a certain meaning is imbedded, static, so, too, the attempt to keep consciousness narrowed down to some particular "attribute" of the "sensation" results in a stultification of the process or the annihilation of consciousness.

We conclude therefore that "attributes" are but some of the meanings which the sensory stimulus may arouse in consciousness. They are the products of a process of abstraction. And if the quality appears to be the "body" of the sensation, this is attributable, we believe, to the fact that the qualitative reaction, the putting over against each other of the various qualities of a sense department is the more habitual one.* The meaning of quality, therefore, when the sensation is observed introspectively, is the one that strikes us most forcibly. On the other hand the day we first experience an inkling of the distinction of saturation from the other qualitative aspects of visual sensation, is a "new" day for us; the color experience is henceforth different from that of former times, and this increase in richness is synchronous with the rise of the distinction. We need not necessarily have names for these new aspects, but the point is

* Cf. above, Section X.

that it is in the analytic process that the new aspect comes to consciousness—and to say that it was always present is to blink the fact of difference for the sake of theory. The coming of the subtler aspects into the field of consciousness dates from the day of their first singling out and their presence comes home to us, often, before we have a name to affix to our new way of reacting to the stimulus. This new way of reacting is the essential thing to be noted. To say that the differences existed before they were noted, is tantamount to taking the position of those who affirm the distinction between psychic reality and conscious actuality (Külpe), phenomena and *Funktion* (Stumpf), content and act (Brentano). (We might add here also Wundt, who speaks of inherent agreements and differences in "psychical processes" on the one hand, and of the "comparing activity by which we perceived" the relations. This latter activity "is different from the agreements and differences themselves and additional to them".)[107]

Our point here is only this: that the actual sensory experience cannot be regarded as being a function only of the physical and the physiological processes of the peripheral stimulation and afferent nervous impulses, but also of the "set" of the nervous system as a whole. If the saturation aspect is to be the "attribute" of the "sensation" as a *conscious* experience, then in the case of those who maintain that the conscious response to one and the same type of sensory stimulus is different before and after they have been put into the attitude in which they become aware of that aspect in a definite way,—this aspect cannot be "attributed" in the same way to the earlier and later sensory responses as *actual, conscious* experiences.

Hypothetically we might suppose that the beginnings of the "attitudes" that are the condition for this modification of experience as sensory, are to be found in certain reflex or instinctive responses.* These, accompanied by certain emotional reactions, might thus affect the machinery of consciousness. But this would be quite other than an "immediate" cognitive reaction by way of

*See below, Section XXXIII.

the cortex to this particular aspect, and would probably be denied by all who deal in elementary sensory "ultimates"; yet it may well be that such a statement of the psycho-physical processes involved in sensory experience will some day come to be regarded as being more serviceable than our altogether too simple conception of psychical element.[110]

The "sensation", that by definition "comes into consciousness only and always with all its attributes", is a logical construct, in the same sense in which every other "ideal object" is a logical construct, and psychologically it behaves just as every other "complex" object. It is in the conscious reactions now to this problematic situation, now to that, as they arise in the breaks in more or less automatic habitual responses, that the various "attributes" that constitute the sensation come to consciousness. The admission that within the "sensation" itself there may exist two degrees of clearness, is sufficient to make it complex. Its very existence is dependent upon the functioning of a selective activity and it depends upon the situation what aspect is to be selected out. And since the "sensation" turns out to be a complex object, that behaves like every other object that may come to consciousness with different shades of meaning, then the dwelling upon it, in the sense of "inspection", is impossible without interfering with the "process" as it normally would go on, for it must result either in bringing out new "meanings", aspects that were not in consciousness in the same way before, or else, as in the case of all attempts to keep "meanings" static, in an annihilation of consciousness.

XXI

Pursuing this line of criticism we come to believe that not even in the case of the "sensation" with its attributes can we introspect in the sense of inspection without interfering with, or modifying, the on-going process. To observe a sensory experience results in putting the "sensation" into a new setting. The

system of associations into which it is received is quite other than that in which it existed in the original experience.[99] Yet one might say that that is the very thing that was wanted. However differently we may have reacted to the stimulation under other conditions, it is under the guidance of a psychological purpose that one would discover what the facts concerning the simplest sort of conscious reaction to a given stimulus "really" are. Let it *mean* psychical element, if you will, but in narrowing down consciousness to the sensation experience, and noting that it, after all, was complex, have we not found a type of process that was about as simple as could be, viz., the various attributive aspects of sensory experience? And further, one might say, given a psychological purpose to start with, we could then have the conditions necessary for attending to a psychical process that was elementary, simple, in the first place, and secondly was one into which introspection would not be introducing changes while the observation was in progress. In other words we should have the conditions given for a case in which introspection would be very like inspection. . . This would verily be a case in point. The conditions would be very similar to the case of the attempt to induce hypnotic sleep cited a while ago. Only here, the command from without is replaced by the purpose within. Yet the two have this in common: they would function effectually in cutting off all avenues of association excepting the one that means: a particular sensation attribute. Having thus stultified mental activity by means of the purpose, we fix upon the attribute; and having fixated it, what next? . . . We try to get it over and over again, just sheer awareness of quality, say. We cannot vary the monotony by noting the other attributes that we have been accustomed to note with regard to a stimulus, so the apprehension of the one aspect simply repeats itself. We do not deny that we are constructing a hypothetical case, yet we have some basis for it in concrete experience. Who has not at times sat staring at the wall of his room, his mind a "perfect blank" except for the awareness of the color of the wall? The color would become clear in repeated perceptions, along with the name, perhaps, as an auditory

image, or as an incipient enunciation of the word. There appears to be somewhat of a perceptual pulse,—whether or not this be a function of the "rhythm of attention", so-called, need not detain us here. Another instance from the class-room may also be a case in point. We go to a session of a class immediately after partaking of the noon-day meal. We are drowsy and would doze off if it were not that we might be called upon to respond, and that bit of "knowledge" is more or less effective in keeping us awake. But if it be a lecture, we have a more difficult time of it. We soon cease to form the associations that it is the lecturer's purpose to call up in our minds. The effort is too great, especially if it be a technical mode of expression that is being used. The words soon begin to strike us merely as sounds. But for the sake of courtesy we try to keep up the appearance of attention, and we come back to the fading auditory stimulation with a jerk. We shall not here try to analyze out the difference between those "comings back" that carry with them the "feel" of being a function of the stimulus, and those that appear somehow to be due to the inner pulling together resulting from a vague appreciation of the social situation. Suffice it to say that purpose and stimulus each appear to influence the pulses of perception each in its own peculiar way. Now the sound seems to have come back of itself, now it appears to have been called back. These meanings appear to attach to the percept; they seem to be incorporated into the experience of the moment. But the point to note is this: in the successive pulses of perception the sound consciousness becomes ever vaguer and vaguer, and we are soon off in the land of nod. This we realize only after a well-intentioned neighbor has pinched our arm.

To return to the hypothetical case, in which the observer is required to fix attention upon one aspect of the sensation and to continue to it. Here we do not alternate, as in the case of staring at the wall, between the more "fundamental" quality aspect and the subtler intensity aspect, but we stick to quality. Have we reason to believe that the content at the focus will behave differently in this case where the controlling factor is a psycho-

logical purpose, than when consciousness was under no guidance of a purpose to single out that aspect, but responded as a result of habit to this more "fundamental" aspect? In the class-room experience we note the presence of both these factors that might be instrumental in introducing changes into the temporal course of consciousness. In the case of staring at the wall we had not the purpose of keeping the tint "there". Retrospectively, we note that the color came and went and that each time the "jerk" of its coming appears to have become weaker. There was no "pulling ourselves together" to attend. It is only *after* the experience that we become aware of the fact that consciousness had narrowed down to so small a perceptual field and that it had arrived at so low an ebb. . . In what way the presence of a purpose would color this experience need not detain us here—whether as a vague realization of the situation, or as expressed in voluntary control of the process of accommodation of the sense organ, or in the mere "feel" of the accommodatory adjustment. But if it should be found that when this inner factor is present the sensory process at the focus takes a different course than when it is not involved, then we would once more have in this last instance of observing a single aspect of sensory experience likewise a case in which the attempt to introspect it while it is in progress, introduces changes into the "process" itself. . .

Now introspectively, we believe, the way of staring blankly at the wall—with the bare awareness of the color quality as the sole "object" at the focus of consciousness—is different in its course, at least, from the experience in which its perception is under the guidance of a purpose. The color consciousness in the first instance might be described, perhaps, as a case of simple apprehension; the pulses of apprehension have a certain quality of their own. The second case, of trying to keep the stimulation in consciousness, has in it all those elements that differentiate "active" from "passive" attention. But in both cases it might be rejoined, it is the context that changes, and not the content at the focus. In our hypothetical case we might have both types of attention present, the content coming in now in such a way that

its coming-in has the "feel" of being a function of the stimulus, and now again in such a way that it has the "feel" of being a function of the central factor. But the content itself, some might say, the quality that is perceived, is not altered by it. We would answer that even though this should be shown to be true, the process, the temporal course, the duration aspect, is different under the influence of a purpose from that in the case of a passive awareness of the object. If then these differences between active and passive attention exist, then must we not admit that even here in this case of observation of the behavior off an "attribute" of sensation, the content is influenced by the presence of the psychological purpose to "inspect" the process? The perceptual pulse is no longer that of the coming back in rhythmic flow of a sensory content dependent simply upon the conditions of the stimulation of the nervous elements at the periphery,—but it becomes complicated by those factors that distinguish the course of a process that manifests itself under the conditions of active attention, from that of passive awareness. It is the "process" character of conscious experience that is thereby changed. So long as the psychologist continues to cite duration as one of the "inseparable attributes" of his sensory element, he cannot maintain that the purpose to observe it introduces no changes into the element. And in order to maintain that it is a "real" item in experience, he believes that he must make duration one of the "inseparable attributes", for, says Titchener, one cannot conceive of a sensation that has not some duration. If such an influence upon the temporal course in the attempt to inspect an "attribute" of sensation takes place, in so far as we regard it as a *real occurrence* we must conclude that the presence of the psychological purpose modifies, here, as elsewhere, the on-going consciousness.

But to conclude; as against Titchener our analysis has led us to note that no actual conscious process can be "inspected" without making the purpose to inspect and the judgment resulting therefrom a part of the activity—and when thus injected into the activity it is side-tracked from its course and the end whither

it had been tending. The purpose to introspect, in the sense of "inspect", does verily modify the ongoing process.

As against Stumpf, we conclude that in so far as the concept of phenomena does not possess as a necessary *Merkmal* their being in consciousness they cannot be said to "stand over against us". In so far as they are conceived to become at times "contents" of consciouness, they cannot be said to stand over against us with *all* the likenesses and differences that might possibly be noted but go unnoted. They cannot be said to present more meaningful aspects than are actually discriminated. The conscious experience of a sensory character that occurs in an intensity discrimination cannot be said to be the same as when a qualitative discrimination is made in response to the same inducing stimulation. In the realm of experience as sensory, as in all other realms, enrichment in meaning comes through the discriminative reaction of consciousness. And this is the point of James' contention when he maintains that in no sense can the product of analysis, as a conscious somewhat, be said to be already contained in an earlier experience in which no analysis occurred. The physical stimulation as a physical process may be the same, the conduction path to the cortex might possibly be the same, but this is a very different matter from the statement that the conscious experience arising in response to this stimulation is the same. The lights of the city street of Stumpf's illustration, in so far as we become aware of them as phenomenal content, cannot be said to be the "same" under the two conditions of analytic and of marginal attention. In so far as they were marginally noted as merely so many bits of brightnesses in the field of night, they cannot be said to have been phenomenally the same as when under conditions of analytic attention they are noted to differ in hue, some being the yellow of gas lamps, others the blue of electrics. And we are here remaining entirely within the bounds of Stumpf's own sytem, for we are in no wise referring to differences in the "feel" of *Funktion,* in the two cases, but are restricting ourselves entirely to the phenomenal aspects of the lights.

XXII

The psychologist cannot unqualifiedly assert that it is possible to observe conscious processes from a psychological point of view without thereby interfering with the normal process. To assert that this is possible is to blink the fact of the influence of the central factor, call it whate'er you will, active attention, purpose, *Einstellung,* determining tendency, or what not. It is rather for psychology to recognize that it is at this very point that it can put in its "licks" in contributing its share to the control of our "inner" world, in the same way as the physical sciences have given us control over the world of things. To realize that the psychological purpose is but one of many possible determining tendencies that might be set into operation in determining the "what" of the "that" in consciousness,* is the beginning of psychological wisdom. The "that," the "object," falls into parts that are already habitual ways of conscious reaction. If these fail us, the "that," the stimulus, calls out other, analytic reactions; it is now scrutinized in the light of the way it affects the sense organs, for we must know "what" the "that" that baffles us, "really" is. Hence the sensory analysis. The meanings that come to us in terms of this analysis are the beginnings of the reconstruction of the object, to use a well known functional phrase,—the object that came to consciousness meaning a baffling "that," in the reconstruction under the influence of past experience becomes a "what," and whatever else it might mean besides, now means at least this much: these, and these, and these, "sensations." It is just because the earlier conscious reactions to the "that" were inadequate, that the analytic reaction is called out, and this is the point at which James's contention might be recalled that the analysis into these and these sensations is anything but "contained" in the earlier reaction to the stimulus. Once the psychologist begins to realize that there is such a thing as psychological analysis and observation going on in the normal business

* Compare Bühler's distinction between *Intention* and *Wasbestimmtheit,* i.e., between "thatness" and "whatness"; also Woodworth's distinction twixt sensory qualities and "thinghood." See below, section XXXI.

of life, he will set about studying the rise of psychological distinctions as a part of the process of adaptation, he will seek to note just how "observations" of conscious activities changes them, and thus he will get his point of application in the control of mental process. The natural scientist does not merely describe, he seeks to control. The chemist does not merely want to know about chemical elements, he wants to be able to make them do his will, and as a result we have the impressment of chemistry into the service of human ends, just as the physicist's mastery of matter gives to humanity the control of the world in terms of physics. So, too, biology is attempting to develop eugenics as a science. In like manner psychology can render its quota to the control factors in human life by showing up the mechanicism by which changes are introduced into the course, the flow, of consciousness. To note that the psychologist can introduce changes in consciousness, can "interfere" with it, is the fact to be "pounced upon", and studied. He should ask how are these changes brought about. A body of knowledge of how to introduce these changes is the beginning of a new era in human culture. We instanced above the rise of the habit of dissociating the sensory stimulation from its meaning, the habitual conscious reaction from its stimulus. We noted that the modern mystics are making this the central feature of their method. Now an adequate account of this method, of this psychological process of dissociation for the purpose of allowing new associations to rise, would give us an element of control in our psychic life that would be of value not merely for mysticism, but more broadly for science and the affairs of common life. It would give us the tool by means of which men might literally limber up their minds. . . It is such a body of knowledge, also, that a truly scientific psychotherapy must make its point of departure.

We note then that our provisional criterion that we sought to apply to the types of experience designated by various categories in our psychological system, for purposes of classification, fails us. We cannot classify on the basis of conscious states that suffer and those that do not suffer immediate "inspection". We

saw that the injection of the psychological purpose into a normal activity inevitably interferes with the on-going process, i.e., differentiates it from an otherwise similar process into the course of which no such psychological purpose is introduced. This means, then, that the "taking of mental notes while the observation is in progress, without interfering with consciousness", is impossible. It means further that Stumpf's characterization of the "phenomena" that "stand over against us as somewhat objective, that seeks merely to be described and acknowledged", does not apply to anything in the way of actual consciousness that we have been able to discover.

XXIII

We now come to the question: How have we come by the category of a static mental "element," a somewhat "that stands over against us?"

There is an ancient philosophic distinction between "sense" and the "understanding". The senses were supposed to furnish the raw material which the understanding works over into ideas, and these in turn into the concepts that constitute our mental furniture. And what is more, we find that there are some among us who believe that this working up is not merely inferred when we find the furniture there in consciousness,—no, we may also become aware of the very process itself, we can hear the whirring and buzzing of the manufacturing, as it were. The raw material that comes in, is conditionel in large measure by the constitution of the sense organs and by the manner in which they are affected by the things of the outer world. But whatever the metaphysical conception of the status of the in-coming material may be, it is supposed to possess a certain fixity of inner constitution. It is the phenomenal reflex into consciousness at the point where the the individual comes in contact with the "world" in which he lives. It is not the "world" itself that comes in, nor a duplicate copy of it, but that which comes in is an "appearance", phenom-

enon. Over against this phenomenal content is the understanding. Spinoza and Descarte's distinction twixt extension and thinking recurs in Stumpf's categories of phenomena and *Funktionen,* for "*das uns gegebene Tatsachenmaterial zeigt eben schon in der Wurzel ein Doppelantlitz*".[55] The philosophic ancestry of the distinction throws some light on certain features of contemporary conceptions.

The modern sensation is in part a descendant of one side of this duality. We shall not here trace the line of descent,* but would point out that it is discernible, for instance, in the statement of the sensation as the sum of all its attributes. To it still adheres much of that relatively fixed character that belongs to the phenomena over against the more labile thought ingredient of consciousness. And in Stumpf's making the fact of being in consciousness not a necessary *Merkmal* of the phenomena we see a logically consistent carrying-out of the distinction. The phenomena need not necessarily be in consciousness in order to constitute them phenomena. The understanding, in the case of Stumpf's system, one of the psychic *Funktionen,* notes elements here and others there, i.e., focuses upon them. The phenomena are there, waiting to be acknowledged and described. Hence the doctrine of the possibility of unconscious phenomenal contents.[66] The fact of being noted, the fact of coming to the focus, in a word, the fact of clearness, is not, in such a system, an attribute of the phenomena, but of the process of apprehension. The phenomena may remain the same when apprehended marginally as when attended to focally. Relations and likenesses and differences exist between the phenomena that may or may not be apprehended in the operation of the psychic *Funktionen.*[56] Phenomena and *Funktionen* are independently variable. The peppermint experience before analysis and the peppermint-analyzed-into-its-elements have the same phenomenal content—what has changed is the *Funktion.* On the other hand, the *Funktion* may remain constant whilst the phenomenal content changes, as when, lost in thought, at the twilight hour, I am still aware of my surroundings through the

* See below, Section XXXIV.

visual impressions coming in. I give them, presumably, an unchanging degree of marginal attention, i.e., the *Funktion* remains constant, so, too, the meaning that accrues to the phenomenal content; but the phenomenal content that is apprehended is changing without my becoming aware of the change.[66] These are examples illustrative of the thesis that phenomena and *Funktionen* are independently variable. What remains the same and unaffected by these phenomenal differences is the *Funktion,* in Titchener's terms the "attitude" that we have assumed in response to the stimulation arising from the objects gradually changing under the influence of twilight illumination. It is assumed that certain conscious *phenomenal elements* are likewise changing, but the "clearness" of apprehension with reference to them, and the manner in which they are apprehended (the "pattern" into which they would have to fall as a result of the activity of the *Funktion des Zusammenfassens*) may remain constant. Such a characterization is possible only in a system in which sensory phenomena with all their "attributes" may exist in consciousness without being meaningful. Just bare attributive changes are here supposed to have been going on, presumably within consciousness, and yet called out no meaningful reactions. In the phenomena relations of likeness, of difference, etc., may exist without being noted. There are even unnoticeable differences;[66] and nothing would appear to stand in our way if we would posit, say, sensations that remain below the threshold. Furthermore, "the fine distinctions in the content of the sensations that are ours are not always directly present to us. We must differentiate once more between phenomenon and thing-in-itself within the realm of the phenomena themselves."[67]

So much for this conception of sensory phenomena as over against the understanding. The world of phenomena is not the world of physics, nor yet is its existence dependent upon the presence of the phenomena in consciousness. We study the phenomena under conditions of focalized attention, but the attributes that we discern in them and the relations existing between them are in no sense dependent upon consciousness for their

existence. Stumpf would make the science of phenomenology a discipline of its own.[69a] Phenomena are the starting point for both physical and psychological science, but only the starting point, for the real matter of the sciences lies to either side of the phenomena. The real business of psychology is the study of operations of the *Funktionen,* and he carries out his conception consistently within the limits of his system. At one point an interesting question might arise: Are we to conceive the transition from focally apprehended sensations over into marginal and subliminal sensations as a gradual decrease in clearness of apprehension merely? And if so, would the hypothetical sensations below the threshold have at least physiological representation,—specifically in the same parts of the nervous system as the sensations that have conscious existence, i.e., in the cortex? If subconscious phenomenal representation be denied to stimulation that functions in releasing subcortical reflexes, the distinction would appear a trifle arbitrary, for it would rest on purely physiological grounds, i.e., the distinction between unconscious cortical paths and subcortical paths. If, on the other hand, the distinction be not drawn somewhere, then all stimulation of the physiological organism might claim sub-liminal representation in the phenomenal nether-world of the psyche. Thus it would not be an illegitimate interpretation of Stumpf's illustration to affirm that the stimulation arising from the presence of the lights of the city streets functioned a large part of the time in releasing purely unconscious reactions resulting in my keeping to the path as part of the total adaptive process in an accustomed environment. On the first supposition viz., that only cortical stimulation is to have phenomenal representation, whether conscious or unconscious, the lights would at times, under a focal and marginal apprehension, have conscious phenomenal representation,—at times merely sub-liminal representation, in so far as the reactions resulted in the form of unconscious cortical reflexes, and at other times no phenomenal representation at all, whenever the cortical reflex should become short-circuited by way of a thalamic or even some lower correlation center. Yet this is not the interpretation that

Stumpf would seem to make in his illustration, for he says: "Surely, we must say to ourselves that just a moment ago there also were light and sound impressions of the same sort and in the same spatial and temporal relations as we now perceive them," i.e., under conditions of focalized attention. The reaction of "just a moment ago" may have come under any one of the three neurological possibilities just mentioned, including the case where the stimulation does not release a cortical but a sub-cortical, let us say, a thalamic, reflex. If we are right in this, then stimulation functioning in releasing not cortical but sub-cortical reactions also might claim sub-liminal phenomenal representation. But the moment we take this position it means that we must allot to all stimulation of the physiological organism, of whatever sort, whether from within or without, phenomenal representation. We would thus obtain a wealth of phenomenal content; but whether any rational need justifies us in affirming that the unapprehended sensory materials that by hypothesis are to be assumed as the unconscious phenomenal representation of stimulation that functions in releasing automatized sub-cortical reflexes,—whether we are justified in affirming that such hypothetical sensations are "the same" as the sensations of conscious experience, is open to grave doubt. . .

The phenomena, then, do not exist in consciousness as relatively stable "thats" that may be inspected. In so far as they do come to consciousness in the concrete experience they are inextricably bound up with the *Funktion* of apprehension, awareness, or what not, and it is only by logical abstraction that, in the first place, phenomena and the apprehending *Funktion* may be differentiated, and, in the second place that the phenomenon, say a specific senory experience, can be characterized as a somewhat that stands over against us that asks merely to be described and acknowledged. Such a characterization of the phenomena is a result of logical abstraction and it is in no sense true of the actual conscious experience.

Such being the philosophic antecedents from which springs the present day conception of "sensation", the descendant of that

which is "apprehended" by the "understanding",—it is small wonder that we get into difficulties when we try to "attribute" to the sensation some of the characteristics, properties, or what not, that in that ancient distinction had come to be assigned to the other side of the division, as when, for example, we try to make the fact of clearness, the way in which the "understanding" apprehends the "phenomena", an attribute of the phenomena themselves. With the reaction against the "faculty" psychology came the desire to state consciousness in purely phenomenal terms,—to state it "as it is, existentially". The sensations of our day are the phenomena of the ancient distinction, reconstructed to suit an attempt at a purely structural statement of consciousness. They seek to cease being the rigid entities that they once had been while still a part of the dual system, and take upon themselves some of the characteristics that in those older systems and in the contemporary systems of Stumpf and others are attributes of the *Funktionen.* Yet when we are mindful of the origin of the abstraction we can somewhat more readily appreciate the difficulties into which we are thus led. Taking up into our definition of sensation the statement that sensations never mean—a function that in the dual system had been abstracted from the conscious experience and attributed to the understanding,—and having taken over this *Merkmal* of being meaningless, and at the same time eliminating from our psychological system that which erstwhile performed this function, we find that our method of procedure works havoc within our psychological conceptions themselves. We call the sensation a "process", hoping thereby to emphasize the fact of change and to eliminate the rigidity of the "phenomena". Yet logically we defined a sensation as the sum of all its attributes, and so long as we retain such a definition, a change in any of these attributes makes the sensation *ipso facto* another sensation, so that we do not get *a sensation* that is *a process,* but *a series of sensations* that might, perhaps, constitute a *process.* But it can hardly be said to constitute that so long as we retain the definition, given above, and the postulate of sensations as meaningless;[80] for by the definition

each group of "attributes" constitutes an element in itself, and the postulate prevents the consolidation of these into a "process", for to do that would be to establish relation between the various parts of the series, and the introduction of relation would constitute meaning. For the various parts, or elements, of the series would be each other's context; and context is that which constitutes meaning;[80] hence a process in which changes occur would necessarily be itself a meaning. We have already noted how the attempt to make the fact of clearness an attribute of sensation, leads to other logical incongruities. . . Thus we see that the attempt to give an intelligible account of consciousness in terms of the "phenomena" that, however we may seek to alter the conception, betray all along the line their one-sided origin in that earlier abstract distinction between "sense" and "understanding", —that such an attempt leads us into hopeless confusion. In its continual insistence that it is a "process" and not a static somewhat, the modern sensation reminds us of the words of the Queen in Hamlet: "The lady doth protest too much, methinks."

On the other hand, the abstraction of the "process" character, of the "active powers", from the "phenomena", leads in other systems to a hypostatization that is equally illegitimate or equally legitimate, as one chooses. Here very soon "act" and the meaning that accrues to a situation by virtue of the act (for the "phenomena" are without meaning), came to be differentiated, and as a result we have not merely *Funktionen* over against phenomena, but also the meanings that are the "correlates" of the *Funktionen*. These correlates are the *Gebilde* in Stumpf's system.[64] In other systems there are analogous concepts, such as *Formen, Gestaltsqualitäten,* etc. In Stumpf they are not conceived of as quite so independent of the *Funktionen* as are the phenomena, but when we turn to Bühler's discussion of Stumpf's system, we find that the conception of the *Gebilde* has already come to partake more of a content character and is subsumed by Bühler under the category of *gedankliche Inhalte,* thought contents, as over against the *Empfindungen* and *Vorstellungen* that constitute the *sinnliche Inhalte,* sensory contents.[22] And differen-

tiation once started, there is no end of new categories: *Bewusstseinslagen, Regelbewusstein,* attitudes, *Bewusstheiten,*—to name but a few. In how far these are *Funktionen* that are in consciousness, yet are chary of being called *Inhalte,* contents, but nevertheless exhibit some leaning in that direction,—there is little unanimity. This chariness on the part of the *Funktion* to be classed with the *Inhalte* and its protestation that it is nevertheless "immediately given" is sufficient earnest to us that it is the counterpart of the activity phase, of the process character, abstracted from the totality of conscious occurrence. The manifold of new categaries is truly staggering,—and all protesting vehemently that they are "non-senory" or "imageless" or "pure" thought or activity. Scant courtesy is paid the Sensation by those who have transferred their allegiance to the new god, and surely we must take off our hats to the few trusty fighters still defending the altar of the venerable Concept of Sensation, that since Locke and Hume, has been regarded as the liberator of mankind, as the beacon of empirical science, as the "ultimate" of human experience.

XXIV

Yet in the minds of some the question may arise whether anything is gained by the invocation of new categories to be characterized largely in negative terms as "non-sensory," so long as we are in a state of doubt as to the positive content of the concept: sensory. It is only when we have a more or less definite meaning to attach to "sensory" that we can be sure of what the newly introduced "elements" or other categories are *not.* In other words, we would begin a definition of the problem by a re-examination of the relation of the concept: sensation, to these other "non-sensory" categories as it comes out in a comparison of several recent writers on the subject.

We have noted that in the actual experience of everyday life not all aspects of "sensations" are of equal importance, sometimes

it is the qualitative aspect, sometimes the intensity character, that looms large; and we further pointed out that the determining factor lay in the situation as a whole. In each specific case it might be a function of instinct, habit, or of purpose. In the words of Angell, "we shall always find that this sensation is determined by the demands made upon the organism by the environmental situation, i.e., that it is functionally determined and that it will vary with each specific situation with which the organism will have to cope. . . . It is never a mere sensation in general. It is always this *specific* sensation produced by certain particular, momentary organic conditions."[5] Even though the stimulus be the same, the actual conscious character of the sensory aspect of experience that arises in response to the stimulus will vary from time to time as the situation or the purpose varies. "One may of course hypostatize this sensation and, dissociating it from its particular surroundings, regard it as a type of a relatively static structural element, for which specific function is a secondary and unimportant consideration. But the actual sensory experience which constitutes the prototype of this hypostatized sensation, is not only capable of being viewed as an expression of functional activities, it cannot be correctly viewed or accurately described in any other way."

A number of writers dwell upon this point that the actual conscious character of the sensory experience is dependent on the functional activity that is going on at the time being, and that it is not a fixed, staring, stable, static, immutable somewhat. Yet most of them are chary of departing from the venerable dogma concerning the "inseparability of the attributes"; instance recently: Watt, in *The British Journal of Psychology, Volume IV.*, Part 2, on *The Elements of Experience and their Integration;* or *Modalism,* and Aveling, in the same volume, on *Relation of Thought-Process and percept.* Both emphasize especially the variability in the relative functional importance of the several attributes. That the various "aspects" may vary from time to to time in attentional clearness even Titchener teaches. And Meumann, likewise, writes as follows: "Beachten wir die Inten-

sitäten, so treten die Qualitäten, die räumlichen und zeitlichen Verhältnisse für unser Bewusstsein zurück, beachten wir räumliche Verhältnisse, so gilt dasselbe von den Qualitäten, Intensitäten und Zeiten. Beachten wir die Zeitverhältnisse, so treten alle qualitativen, intensitiven, räumlichen Theilinhalte aus dem Blickpunkt des Bewusstseins; beachte ich Muskelpannungen ihrer Intensität oder Qualität nach, so verschwinden relativ für mich ihre zeitlichen Verhältnisse."[40]

But when we turn to Külpe, we find that he comes very close to cutting the Gordian knot in affirming the separability of the attributes for "actual consciousness", even though they continue inseparable in some strange nether-world of "psychic reality".

In connection with an account of certain experiments on abstraction, Külpe notes this fact of difference in the sensory aspect of experience in so far as it is a function of the dominating purpose of the moment.[34] In these experiments groups of symbols were presented, the elements of which differed among themselves in form, in color, and in arrangement. Instructions were to focus now upon the number of elements in the group, now upon their arrangement, now upon their form, now upon the colors. Concerning the different ways in which the content was in consciousness under the influence of the various types of instruction or purpose, he says: "Für die Erklärung ist zunächst wesentlich, ob die gefundenen Unterschiede . . . auf Unterschiede der Gesichtsempfindungen order der appercipierenden Faktoren zurück zu führen seien. Werden Z. B. die *Elemente* oder die *Farben* anders gesehen, wenn entsprechende und wenn heterogene Aufgaben vorliegen, oder werden sie anders aufgefasst, ohne dass die Gesichtsempfindungen selbst in beiden Fällen einen erheblichen oder wesentlichen Unterschied darboten? Darauf kann, wie ich meine, nach unserem Protokoll und der ganzen Versuchsanordnung nur gesagt werden, dass der Unterschied lediglich oder doch wenigstens der Hauptsache nach in der Auffassung, nicht aber in den Empfindungen liegen kann. Mag ferner in manchen Fällen ein rasches Vergessen stattgefunden und die Aussagen über die der Aufgabe nicht entsprechenden

Teilinhalte beeinträchtigt haben, so ist doch zumeist, wie ich auf Grund des Protokolls feststellen kann, die Auffassung selbst unmittelbar eine andere gewesen für entsprechende und für heterogene Aufgaben . . . Die Farben erscheinen (in the case of "*heterogene Aufgaben*", i.e., in problems in which the question of color was irrelevant) tatsächlich nur als gleich oder verschieden, als dunkel oder bleiben ohne Ortsbestimmung . . . Am stärksten zeigt sich die apperzeptive Natur dieser Tatsachen darin, dass Aussagen über *Elemente* oder *Farben* überhaupt, in jeder Richtung unterblieben. Die Versuchsperson ist z. B. im Stande eine Figur richtig zu beschreiben, ohne über die Beschaffenheit der sie begrenzenden Objekte irgend etwas unmittelbar im Bewusstsein erlebt zu haben."

"Ich lege wert darauf zu konstatieren, dass in den Abstraktionstatsachen unmittelbare Bewusstseinsphänomene vorliegen. . . . Die Versuchspersonen glaubten tatsächlich die Eindrücke in der angegebenen Unbestimmtheit *zu sehen,* bezw. tatsächlich keine Farbe, kein Objekt u. s. w. wahrgenommen zu haben. Da nun die Psychologie als Wissenschaft den Empfindungen regelmässig bestimmte Eigenschaften beilegt, sie aus bestimmten Teilinhalten bestehen lässt, so geht daraus hervor, das sie zwischen den psychischen Vorgängen und dem Bewusstsein von ihnen unterscheidet."

"Dass dieser Unterschied gemacht werden muss, etwa in demselben Sinne, wie man zwischen physischen Vorgängen und dem Bewusstsein von ihnen unterscheidet, dass mit anderen Worten die alte Lehre von einem inneren Sinn mit der dazu gehörigen Gegenüberstellung von Bewusstseinwirklichkeit und Realität für das Gebiet der Psychologie eine zeitgemässe Erneuerung finden muss—das ist das prinzipielle Ergebnis, das ich meinen Versuchen entnehmen möchte. In Anschluss daran definiere ich die Abstraktion als den Prozess, durch den das logisch oder psychologisch Wirksame von dem logisch oder psychologisch Unwirksamen geschieben wird. Die wirksamen Teilinhalte sind für unser Denken und Vorstellen die positive abstrahierten, die unwirksamen aber diejenigen, von denen abstrahiert worden ist.

Für unser Bewusstsein giebt es demnach abstrakte Vorstellungen, für die psychische Realität giebt es nur konkrete Vorstellungen."

We have quoted the section at length because we believe that it contains much that will elucidate the relation between "sensation elements" and the "non-sensory components" of perception and thought, and also much that will bear on the problem of content and function. Let us note first that when the sensory *qualé* is irrelevant to the problem, the subject believes *tatsächlich* to have seen no color, or else the various colors appeared merely as being alike or different (*als gleich oder verschieden*), or else have merely a certain brightness value. In other words, Külpe believes that after all allowance has been made for possible errors due to memory, there is a very actual abstraction from visual quality in the conscious response to a stimulus under the influence of the *Aufgabe* in which the purpose is, say the determination of the character or of the arrangement of the elements or units in the presentation. The quality actually does not enter consciousness, yet there may be some awareness of brightness or of sameness or of difference. The subject may be able to "describe accurately a figure or contour, without having had any immediate conscious experience of the attributes (*Beschaffenheit*) of the delimiting objects". We believe that we are justified in interpreting this as meaning that what comes to consciousness under these specific conditions created by the *Aufgabe* is something of the nature of *Gestaltsqualität,* and that, while none of the "attributes" of the delimiting and of the delimited fields came to consciousness, the awareness of some difference between the two became the basis for the apprehension of the quality of form . . . In the conscious reaction, then, there is an abstraction from irrelevant aspects of sensory experience, that under other conditions, specifically under the influence of another *Aufgabe,* might have come to consciousness. Such abstraction would appear to be all of a piece with those cases of everyday experience in which the intensity of the stimulation appears to come to consciousness abstracted from the specific quality, as when a bright light is suddenly flashed before us on a dark night,—the case on which we dwelt some time ago.

If this be true, then we believe that we are on the track of the distinction that Külpe and others make between *Bewusstseinwirklichkeit* and *psychische Realität*. He tells us that the differences in the content that are noted in the different reactions to the stimulus resulting from the change in purpose, are to be characterized not as differences in the sensations, but as differences in our apprehension of them. For, "since psychology as a science regularly assigns to sensations certain attributes, allows them to be constituted by certain part-contents, it follows that we must distinguish between psychic processes and our consciousness of them". The "psychic processes" in this case are the underlying sensory processes, and, we take it, these processes, as sensations that are "the sum of all their attributes", are to be postulated as "psychic realities" but not as "conscious actualities" in this series of perceptual experiences. We would not impute to Külpe a point of view that may be foreign to him, yet it may not be amiss to indicate here a point of contact that he would seem to have in common with functional psychology. He says, "I would define abstraction as the process by which the logically or psychologically efficacious (*Wirksame*) is separated out from that which is logically or psychologically inefficacious (*unwirksame*). The efficacious part-contents are for our thinking and ideating (*Vorstellen*) those that have been positively abstracted, the inefficacious those that have been abstracted from. For our consciousness, therefore, there may exist abstract *Vorstellungen*, for psychic reality there can be only concrete *Vorstellungen*." Sensations and images as they come to consciousness are here referred to under their aspect of *Wirksamkeit*, efficacy. It is the function that they are serving that determines their conscious character. Those aspects (*Teilinhalte*) come to consciousness that are germane to the solution of the problem. It is the *Aufgabe* that determines how the objective stimulation is going to be reacted to by consciousness. The inefficacious aspects are abstracted from; they need not even come to consciousness marginally.

Sensory experience would certainly appear to be regarded here from the point of view of function. A like significance

would seem to attach to the characterization of consciousness as actual, *Bewusstseinwirklichkeit,* as over against the psychic reality (*psychische Realität*) that must be ascribed to the sensations as they are "in reality". The distinction could not have been clothed in these words haphazardly by Külpe and therefore the choice of the word "actuality", *Wirklichkeit,* is significant on account of its functional import as over against the more static meaning of *reality* that is ascribed to the "merely" psychical. Here, then, we would at least seem to have common ground with the functionalist so far as we are dealing with "conscious actuality" and not with an abstract "psychic reality". For conscious actuality part-contents (*Teilinhalte*), i.e., "attributes" of sensory stimulation, may become abstracted under the influence of the demands of the moment, and constitute the "content" of thought and idea. Such content, then, would have its rise as a response to functional demands and Angell speaks to the point when he says that "it cannot be correctly viewed nor accurately described in any other way". It is merely a matter of terminology when Külpe calls the "part-content" thus functionally effective "abstract"; in this sense all content of actual consciousness might be referred to as "abstract", but this would involve us in the larger question whether all consciousness is essentially selective and we still have to face the problem of the images that have the appearance of mere "by-play" and of "sparks struck off by thought in its progress".[100] Here let us dwell only upon the implication that form or arrangement sensory *qualé,* intensity, quantity, may each be "abstracted", and become the effective content of actual consciousness. Sensory experience in actual consciousness, then, is not a mechanical sequence of sensations as they are described by "psychology as a science", but it is rather a looming up of now this aspect, now that, all in response to needs arising in the on-going activity

XXV

Külpe states that the differences in the varying perception of the presentative material, parallel with the variation in the *Aufgabe,* are not differences in the "sensations" (*Empfindungen*) but differences in the mode of apprehension (*Auffassungsweise*). We are here face to face once more with the dual system with its distinction of sense and understanding. The "inner sense" selects, apprehends in some mysterious manner, the "psychic processes" of sensation, and it is only so that they are lifted from the plane of mere "psychic reality" to the level of "conscious actuality".[35] With such a dialectic, to affirm that the "sensations" remain the same, Külpe can mean only that the stimulation is the same, and such a statement contains no reference whatever to characteristics of sensations as "contents" of consciousness. The sensation has "psychic reality" ascribed to it; yet it would save a world of misunderstanding if we would treat it frankly as a physiological category. And yet Külpe speaks of the concrete *Vorstellungen* of psychic reality as over against the abstract *Vorstellungen* of actual consciousness. We would ask: Is it possible for us to experience the concrete *Vorstellung,* or not? It appears almost futile to discuss this point until we are given further information concerning the character of psychic reality, but it is necessary to appreciate the difficulty, at least, before we can go on to a discussion of "imageless contents". The hypothetical psychically real sensory processes that are manipulated in sundry "acts" of consciousness, are evidently regarded in some sense as "given", before they are apprehended by consciousness. Then under the influence of the purpose there is in the process of apprehension abstraction from irrelevant psychic reality. The relevant comes to actual consciousness as the functionally effective content. But nevertheless a part of the constitution of the *Vorstellung* is conceived as being peculiar to itself and not attributable to the action of the apprehending act. Intensity and sensory quality belong to the "sensation" in psychic reality, even though either of these aspects may be abstracted from by actual consciousness. For purposes of statement within a psychological

system this statement of psychic realities that are apprehended by an act of consciousness might have some justification, but unless carefully qualified the impression might arise that somehow there are certain "sensations" coming to consciousness and that "parts" of these, *Teilinhalte,* are then apperceived in an "act" subsequent to their appearance. The ascription of "psychic reality" to these hypothetical sensations and their attributes prior to their appearance in consciousness tends to increase the probabilities for the operation of this source of error. One thing to note is this: Whatever the constitution of the hypothetical sensations of merely psychical reality, the sensory experience of actual consciousness is not to be conceived as their copy. We must recall always that these hypothetical sensations are logical constructs and that they are in no sense brute "givens", but are the postulates of "psychology as a science". Sensory consciousness may be varied, yet the "sensations" remain the same, so long as the objective stimulation and the conditions under which it operates, remain constant. "Sensation" and "consciousness of sensation" are two categories that must be carefully kept apart, and it is here that we must locate much of the cause of misunderstanding in the non-sensory imageless thought controversy.

We note then that the category of sensation for certain psychologists is not a category of consciousness at all. Actual consciousness for such psychologists as Stumpf and Külpe is conceived as being essentially functional. "Sensation" is subsumed under the category of phenomena, into the conception of which the characteristic of "conscious" does not enter as an essential *Merkmal,* or else *Empfindung* is characterized as a "process" of "psychic reality", which, undefined as it is, has the negative differentia, at least, of being put over against "conscious actuality". While there may be eminently good reasons for doing this, it would save much misunderstanding to make it frankly a physiological category; then it would be more easily detected when it got mixed up in explanations in which it masquerades as a conscious somewhat.

XXVI

To return now to "conscious actuality" and to the abstract *Vorstellungen* that are found there. We saw that this conscious actuality as a whole and the abstract *Vorstellungen* that appear in it as part contents were characterized by Külpe in terms indicative essentially of functional efficacy. It is in this consciousness that we gain that experience which, under the influence of a psychological purpose, we later systematize and as a part result may obtain the conception of a relative static structural element that we hypostatize as the "sensation" or the "image" of psychology. But in the meantime conscious actuality continues to do business with the "abstract" (?) shreds of sensory experience and does it in a wonderfully efficient manner. The psychologist, having finally laboriously constructed the "real sensation", comes back to conscious actuality and is dissatisfied with his "element." The element that he has constructed will never do for purposes of describing what goes on in this "conscious actuality" (*Bewusstseinwirklichkeit*). Indeed, he becomes convinced that there is "something more" in consciousness that is nothing like the concrete *Vorstellung* of psychic reality, postulated by psychology as a science. So he thrusts aside his construct, the sensation and image of psychic reality, and proclaims that conscious actuality is largely "imageless" and "non-sensory". Forsooth, the sensations and images seldom enter this consciousness, and when they do, they are "troublesome", they hinder rather than further thought. Indeed Woodworth notes that they partake essentially of the nature of "by-play", that they "have more the appearance of sparks struck off by thought in its progress than thought itself". [102 26] Or again, Bühler concludes that "a look into our protocols suffices to say that anything that appears so fragmentarily, so sporadically, so occasionally, in consciousness as the *Vorstellungen* in the course of our thought experiences, cannot be regarded as the carrier of the closely-knit and continuous content of thought".[12]

XXVII

And what is it that our psychologist abstracts from experience as the new "element" to put over against the "sensation"?

We saw that color quality, according to Külpe's results, may be abstracted from. *"Die Versuchspersonen glaubten . . . tatsächlich keine Farbe.......wahrgenommen zu haben."* The color qualities do not come to consciousness; *"die Farben erscheinen tatsächlich nur als gleich oder verschieden"*, or else their white values find representation, they appear merely as *dunkel*. But the factor that may come to consciousness in these cases is the figure i.e. the form or arrangement. We might conveniently refer to this as the form-quality and point out its kinship with the *Gestaltsqualität* of other writers. It is this form-quality that comes to consciousness here in Külpe's experiments, as a result of the operation of a certain *Aufgabe* in response to certain sensory stimulations. The form is the "content" of consciousness; it is the abstract *Vorstellung* which can occur only in conscious actuality, but never in psychic reality, for psychic reality contains only concrete *Vorstellungen.*

Now in all these cases of abstraction the differences noted do not lie, for Külpe, in these *Empfindungen.* He says: "We must say that the differences can lie solely, or at least in the main, not in the *Empfindungen* but in the *Auffassung* (apprehension)." From the making of this distinction we can infer only this: that the *Empfindungen* in which no change occurs, although characterized as psychic processes, are nothing other than the peripheral physiological stimulations of the sense organs. In physiological terms it means that very probably under the different modes of apprehension there is no difference in the activities of the end organs, perhaps also in the activity along the conduction paths up to the cortex, though this might turn out to be questionable even as an hypothesis.[110] But the *Empfindungen* as contributing nothing to these differences are not conscious *Empfindungen.* They have all the attributes that a structural psychology assigns them, but they are not in consciousness. Consciousness is a new function that supervenes. It is only then that the *Empfindungen*

are precipitated into "conscious actuality". In this dialectic the fact of consciousness, then, is what is meant by the "inner sense". Here we have, once more, the phenomena of Stumpf, and the *Funktionen* of consciousness over against them. And now in conscious actuality, under the influence of some purpose, those part-contents of psychic reality are abstracted that are relevant to this purpose. And so it comes to pass that the form-quality may under certain conditions come to consciousness abstracted from the other possible *Teilinhalte.* This is an abstract *Vorstellung* of consciousness as functional (*Bewusstseinwirklichkeit*),—it is the part-content (*Teilinhalt*) that is functioning (*wirksam*) in the problematic situation. I am told to note the form and forthwith the "form quality" is apprehended and all things else are abstracted from, i.e. I perceive form or pattern of arrangement in the sensuous presentation. And for Külpe this actual pattern consciousnes is a product of the process of abstraction; it is not a function of the *Empfindungen.*

For Stumpf this pattern consciousness would be a case of *Gebilde,*—we believe that we may interpret it as such. If so, the fact of falling into pattern would be characterized as *Funktion,* the pattern consciousness itself would be the *Gebilde.* We saw in the course of our earlier discussion that Stumpf held that the Gebilde was not given in the same sense as the phenomena and *Funktion* were given, but that the *Gebilde* was characterized as being the "correlate" of the *Funktion.* It is not a new phenomenal content. Now we saw that Bühler, in a review of Stumpf did a certain violence to Stumpf's system by classing the *Gebilde* among the *gedankliche Inhalte* (thought content) as over against the *sinnlichen Inhalten* (sensory content). But once only does Stumpf refer to *Funktion* as content, and we are inclined to believe that that was a slip of the pen.[56] The logical ordering of the categories of his system is this: The immediately givens are (a) contents, viz.: phenomena and relations, and (b) the *Funktionen.* The forms, *Gebilde,* patterns, are not a third type of given, in the same sense as these other two, but are the "correlates" of the *Funktionen.* What is given need not necessarily be a

"content". For that is immediately given that "strikes one as immediate matter of fact". The fact of falling into a pattern may be, according to Stumpf, a fact in consciousness, but he would probably have us make it a fact of the "how" of consciousness, not of the "what", of content. The correlate of this "how", the *Gebilde,* the pattern, is not, says Stumpf explicitly, a new phenomenal content. It is for this reason that we believe Bühler does violence to Stumpf's system in making the *Gebilde* a "thought content", in spite of the one instance that we mentioned in which Stumpf refers to the *Funktionen* as *Inhalt,* thus putting them on a level with the phenomena and the hypostatized sensations of psychic reality.

It is the form quality, pattern quality, *Gestaltsqualität* that comes to consciousness as abstract *Vorstellung* in the abstraction tests, that Bühler and Woodworth hypostatize as another form of content. The form quality is referred to by Woodworth as percept quality and is the "meaning".[103] And this, as a static somewhat, robbed of its correlation with the *Funktion* in which it comes into being and abstracted from the total conscious actuality, this "meaning", this way in which objective stimulation is responded to by consciousness in the percept, is ranged alongside of "sensations", *Empfiundungen,* as another kind of "content" of consciousness. Thus Stumpf's distinction between phenomena and *Funktion* and Külpe's distinction between conscious actuality and psychic reality are overridden.

We are now in a position to understand more adequately the difference between Stumpf and James upon which we dwelt some pages back: the case of the *naive* peppermint-experience as over against the experience analyzed into sensations, and the case of the actual lemonade-taste versus the lemonade-taste as being "composed" of sensations of sour and sensations of sweet. The way in which the unanalyzed experience "feels", James holds to be a "simple quality". Stumpf denies this; but the bone of contention is not the fact but the psychological naming of the fact. In Stumpf's system the "content" is the phenomenal qualities; in Külpe's it is the "processes" of psychic reality. But the

consciousness of them as "lemonade" or as "peppermint", i.e. the "form" that they take on in actual consciousness, is the correlate of a *Funktion*. The *Funktion* is here the *Funktion des Zusammenfassens,* i.e. the abstraction of the psychologist of the fact that under certain conditions a mass of stimulation is thus apprehended as a unity, which under other conditions might appear as a manifold of sensations. The form is *"das, was eine Melodie oder eine räumliche Figur oder eine sonstige, als zusammenhängendes Ganzes aufgefasste Vielheit von Erscheinungen unterscheidet von einer Vielheit sonst gleicher und gleich angeordneter Erscheinungen, die aber vom Bewusstsein nicht zusammengefasst werden."*[64] The *Funktion,* of which the form is the correlate, is the fact of the occurring, the fact of the cohering; its correlate in each specific case of conscious experience, for Stumpf, is the "form". We believe that logically this is the equivalent of the "percept-quality" of Woodworth, of the *Gestaltsqualität* of Ehrenfels. James' simple quality of perception, then, arises for Stumpf when consciousness reacts upon phenomena in a definite unified way. It has its rise in this reaction of consciousness. It is the correlate of the *Funktion.* Hence to take this reaction of consciousness and to hypostatize it as a new static element would be logically illegitimate for Stumpf within the limits of his system. The lemonade and the peppermint are in actual consciousness what they are experienced to be both for James and for Stumpf, and their difference is not a difference in data, but a difference in working them up into a system. What James affirms is that the sort of consciousness that occurs when I get an analyzed precipitant of "sensations", as they come to consciousness as the result of analysis, is in no sense the same as that of the *naive* unanalyzed conscious reaction. Stumpf's position might be restated in the form that the sensory stimulation that underlies the *naive* experience and the analytic reaction is the same. But only that. The reaction of consciousness, however, and, we believe that Stumpf would have to admit, the whole wide-spread activity in the cortex that underlies this reaction, as over against the activity represented by the afferent path up

to the cortex that underlies the hypothetical phenomena, these are different in the two cases.

This case of the lemonade taste, though seemingly so different, is in principle identical with the case of the abstract *Vorstellung* of the form in Külpe's experiments and the consciousness of the meaning of the spoken words: "Kant's transcendental unity of apperception." All three are alike "sensory", in that their "conscious actuality" is conditioned by some form of peripheral stimulation. To say that they are "non-sensory" is only to affirm that the hypothetical sensation of structural psychology, which is "the sum of all its attributes", that is abstractly bare sensation without meaning, does not enter into this conscious actuality. In this sense the three may be alike non-sensory. But whatever principle be applied for purposes of classification, all three must come under the same category.

XXVIII

We then ask: What is the criterion on the basis of which consciousness is to be characterized as "sensory" and "non-sensory"?

Bühler speaks definitely to the point in the case of *sinnliche Vorstellung*. It is one that can be described in terms of sensory quality and intensity, (*was durch Angabe von sinnlicher Qualität und Intensität beschrieben werden kann.*) Now if the "form" of Külpe's abstraction tests be the type of the *Gestaltsqualität* and of the percept-quality, then it cannot be a *sinnliche Vorstellung,* for it cannot be described directly in terms of visual sensory quality and intensity. "Nothing was immediately experienced consciously concerning the character of the delimiting objects," says Külpe. Hence no direct determination of the abstract *Vorstellung* in sensory quality or intensity. The *Vorstellung* in question, therefore, is not *"sinnlich"* but *"gedanklich"*. That is to say, it is to be characterized as non-sensory on the score of the absence of the hypothetical abstract sensations of "psychology as

a science", the bare sensation that is a content staring you in the face, as it were, whose "being" consists in its "attributes".

Yet in the case of visual pattern or form, even if it be a case of abstract *Vorstellung,* in that there has been an abstraction from sensory *qualé* and presumably from intensity, the question arises: Might not the precipitant in consciousness be characterized as a positive abstraction of the extensity attribute? It is not that we would make a plea for this particular type of procedure, for Külpe does not in his paper characterize explicitly this form quality as *gedanklich.* It is only when we come to Bühler that this occurs. And if the elimination of all the "attributes" is necessary before a conscious experience may be designated non-sensory, then we would ask: Why does Bühler leave extensity out of account? We conclude, therefore, that the only attributes of his "sensory" experience are intensity and quality.

We have as a result this curious situation: All meaningful consciousness that has as its core intensity or quality phases, easily distinguishable, is to be labelled "sensory". But all those experiences that are built upon extensity aspects, such as the pattern or form quality and those that are built upon the order-aspect—what of these? Are they to be regarded as "non-sensory"? And what of the duration aspect of experience, as in the case of various types of rhythm? We point this out only to show that a very fundamental question has still to be faced. If we continue to employ the category of sensation in any form; if arrangement and pattern be in any way referable to extensity and duration aspects of stimulation; and if it be true that in actual consciousness there is possible this abstraction of all these various phases, then, since extensity and duration are usually cited as "attributes" of sensation, it behooves us not to pass them over without mention. It may be, to be sure, that Bühler would still characterize these as sensory as Külpe appears to do, viz. as abstract *Vorstellungen* of conscious actuality. But then Bühler should have named these attributes among the criteria of the sensory. Else he will obtain a curious crossing within his categories, and he can hardly expect the uninitiated to keep them straight. But

having stated his criterion of the "sensory" in terms of the two aspects of quality and intensity, he thus makes possible the characterization of all meanings, whether perceptual or ideational, in which the important factor is order, either temporal or spatial, as "non-sensory".

XXIX

Having thus stated his criterion Bühler does not hesitate to characterize anything in the way of consciousness that might come anywhere near being describable in terms of sensory quality or intensity, as irrelevant. I refer to that aspect of perceptual and ideational experience which even in the absence of the purpose to introspect often comes to us in the form of kinaesthesis or kinaesthetic imagery. It is to this aspect of consciousness, we believe, that a curious quality of "humanness" is attributable that seems to belong to the conscious world of the possessors of this type of mental reaction, as over against the more "external" world of other types of mind. He says: "Man könnte ja auch versucht sein, jene sinnlichen Elemente, die wir kurz als räumliches Richtungsbewusstsein oder Bewusstsein der Änderung dieser Richtungen bezeichnet haben, mit in die Fragestellung hineinzunehmen. Doch ist leicht zu sehen, dass die räumliche Orientierung, welche die Gedanken hier und da zu haben scheinen, etwas so Variables und auch verhaltnismässig Seltnenes ist, dass wir sie trotz des hohen Interesses, das ihr an sich zukommen mag, hier ruhig beiseite lassen können. Auch wenn mir bei einen Erlebnisfortschritt, den man durch die Worte 'aber' oder 'oder' oder 'trotzdem' kundgeben würde, zumute ist, als ginge ich damit von etwas, was rechts vor mir steht, zu etwas, was sich links befindet, oder von etwas vor mir zu etwas hinter mir über, so kann man diese Erfahrung doch nicht ernstlich zu dem Satz verallgemeinern wollen, das reale Bewusstseinscorrelat jener ideellen Kontinuitäten sei in solchen sinnlichen Elementen zu suchen. Dazu sind sie viel zu zufällig und wandelbar."[11] And

this statement is not made in reference to our consciousness of percept quality, but in connection with a more general statement of the relation of this factor to his more elaborate "thoughts", where he is asking: "What are the essential constituents of our thought experiences, i.e., what are the carriers of the thought content: what is the psychically real correlate of the thought that is determined by logic?"[10] Now this kinaesthesis both in sensory form and as imagery that lies at bottom of this form of consciousness, comes perhaps as near as any to forcing itself home upon us in the course of normal consciousness, in the form of "mere sensation", not meaningless, to be sure, as the hypothetical sensation and image ought to be according to the definition of structural psychology, but nevertheless often loosed momentarily from the total perceptual or ideational consciousness in which it was apparently functioning, and flashes for a wee space as a distinguishable percept, a kinaesthetic "sensation". Now, this comes as close as anything to being a "sensation" in actual consciousness, a "sensation" which normally appears, if even there, only in introspective or analytic consciousness. It lies fully as close to the surface in some types of mind as does commonly the "sensation" of smell in the percept of the rose. Yet Bühler eliminates this aspect as "too fortuitous and changeful".[10] He concludes that the more immediately felt consciousness of seeming to go from something at my right to something at my left, etc., in connection with our awarenes of certain relational words, and which in many minds is given in terms of vivid kinaesthesis or kinaesthetic imagery, is to be abstracted from. In doing this he is inconsistent. His problem was ostensibly to discover the carriers of the *Denkgehalt,* "how the function of carrying this *Denkgehalt* is distributed between the *Vorstellungen* (*Vorstellungen* to be defined as above: *sinnliche Vorstellungen*) and the *Gedanken,* and how these two are related to one another."[10] But he forthwith eliminates a great part of sensory experience that may have participated in the actual thought consciousness. At least he does away with the sensory aspect, yet in so far as even such sensory content has been in any way meaningful he

would divorce the meaning and make it a special sort of content, not of sensing or of imaging, but of "knowing" (*eine Bedeutung kann man überhaupt nicht vorstellen, sondern nur wissen*).[21] With this statement all common ground is swept from under us. The sensory or imaginal response of consciousness is conceived as a bare, meaningless, static *what,* so that even in those cases of awareness of pattern or form which might have been abstracted from the qualitative and intensive aspects of presentation in Külpe's tests, we would not have anything that as "sensory" is already meaningful, but something to which a new "element" must be added to make it meaningful. And yet, can it be that the pattern aspect referred to is conceived of as different from the *Gestaltsqualität* which is mentioned by Bühler? In speaking of it he cites an example. "As I look upon the mass of lines of some complicated mathematical figure, at first blush I am at a loss what to do with it, but suddenly something 'lights up' with regards to them. What is it that has thus 'lighted up'? Evidently the meaning of the figure; and this meaning is always something *gedankliches,* in many cases nothing other than its law of construction." "A similar thing occurs when I suddenly comprehend the construction of a machine or the plan of a building."[15]

We believe, therefore, that experiences such as those of the abstraction tests of Külpe and Bühler's examples of *Gestaltsqualität* are essentially the same in kind. "Sensation" would appear to have but little function in Bühler's system; sometimes it may be the 'substrate" of conscious experience; most often, however, it would appear to be quite irrelevant.

XXX

But what may be the relation of his *Empfindung* and *Vorstellung* to his *Gedanken?* What Bühler characterizes as *Gedanken* are not the *Bewusstseinslagen* of Marbe, or the *Funktionen* of Stumpf, or the attitudes of Judd and of Titchener.

No, while recognizing this category, Bühler insists that it is somewhat quite other, viz.: the *Bewusstheiten* of Ach, which he, Bühler would rather term *Gedanken.* They are the *gedankliche Inhalte,* thought content, as over against the *sinnliche Inhalte,* sensory content. We saw that he made his sensory content the counterpart of Stumpf's phenomena, and his thought content the counterpart of Stumpf's *Gebilde.* And we already noted that in doing this latter he does a certain violence to Stumpf's system —for Stumpf definitely states that his *Gebilde,* "forms", were not to be considered as content, but as "correlates" of his *Funktionen.*

Be that as it may, Bühler notes three types of *Gedanken,* (1) the *Regelbewusstsein,*[14] consciousness of rule, of pattern, of construction, (2) *Beziehungsbewusstsein,*[17] consciousness of relation, and (3) *Intention,* which we can best render, perhaps, by the term "objective reference".[18] Bühler's content categories might be tabulated thus:

CONTENT

a.	b.
Sinnlich	*Gedanklich*
1. *Empfiundung* (sensation)	1. *Regelbewusstsein*
2. *Vorstellung* (image)	2. *Beziehungsbewusstsein*
	3. *Intention*

But when we turn to the end of Bühler's section on *Gedankentypen,* we come across another classification of content in terms of *Wasbestimmtheiten* and *Intentionen.* Every content possesses a "what" and a "that", and his types therefore would appear now to have ceased to be independent elements and to have become "moments", aspects of thought. Every content must have a *what* and a *that.*[20] The *what* may be sensory or imaginal, or a *Regelbewusstsein* or a relational consciousness; the *that* is always reference to an object, whether real or ideal. Aside from the consciousness of *Funktion* (Stumpf) or of attitude (Judd and Titchener), which Bühler would appear to subsume under the category of *Bewusstseinslage,* we would now have the following content categories:

a. *Wasbestimmtheiten* (whatness)

  1. Sensations and images
  2. *Regelbewusstsein*
  3. Consciousness of relation

b. *Intention* (objective reference or thatness)

We note then that the "sensation" and "image", when present, may function as one of the "whatnesses" of the thought. In this sense it is made co-ordinate with meaning and relational elements. Thus in the suddenly up-looming meaning of a plan or of a machine, that Bühler mentions,[15] or in the staircase figure of Woodworth,[100] we have such an instance. In so far as we are aware of the whiteness of the paper and the blackness of the lines we have "sensations" as *Wasbestimmung,* i.e. functioning in determining in part at least, the "what" of the "content". In so far as the whole has the meaning that suddenly looms up, the meaning of a particular plan, or in Woodworth's example, the the one or other staircase meaning, we would have *Gedankliches, Gestaltsqualität* or *Regelbewusstsein,* or with Woodworth: percept-quality. In so far as it was an object, a thing, a unified something, it would have in it for Woodworth, thing-quality,[104] for Bühler, *Intention,* i. e. objective reference of some sort.[18]

XXXI

It is this last "non-sensory" aspect of the experience of perceiving the plan, the staircase figure, or what not—that is of special interest to us. Here we are face to face with the problem of the thingliness of things of the philosophers,—the problem that Titchener would have us abstract from as non-psychological. Yet there are others who have the temerity to face it. The problem has been with us throughout the modern period of philosophy. In Locke it is brought in as the "idea of substance" which must be added to the sense impression to constitute the "thing". Yet how does he get it into his system, since as he says it is derived neither from sensation nor reflection?[36] And in

Hume,[28] again, we have a resolute denial of aught but "qualities", yet somewhere there is slipped in unawares a "gentle force" that binds them together. Coming nearer home, we note how not only Bühler[19] posits objective reference as a non-sensory component of consciousness, but likewise Woodworth,[105] attributing a non-sensory quality, "thing-quality", thinghood, or what not, to his perceptual experience, when he says: "The attempt to describe percept qualities as syntheses of sensory qualities is hypothetical in the second degree. The presence of the required images is hypothetical, and no less hypothetical is the power of the images, if present, by combining with the sensation to produce a percept. They might fuse, no doubt! But is the feeling-together of clanging noise and visual picture fully equivalent to the perception of a ringing car-bell? Were the two not felt as attributes of one thing, their mere simultaneous presence in consciousness would not give the percept which is actually experienced." The efficacy of the "gentle force" of Hume is doubted by Woodworth and is replaced by a special element of "thinghood" that must knit together the visual and auditory elements of the experience into a unified whole.

T. V. Moore[41] in his study on abstraction defines perception as "a process of assimilating the data of sense experience to their appropriate mental categories". It is these mental categories that are the counterpart of the non-sensory components of other writers. Among these categories we find that of "something". Theoretically this is believed by him to be the first category developing in the individual mind and it "enters, though not consciously and explicitly, into all his (the child's) later concepts".[42a]

Turning to another contemporary writer, Schultze, we have a procedure similar to that of Woodworth. He posits a non-sensory component: the *Scheinsubstanz,* pseudo-substance, which gives to the experience the object meaning. His illustration is fascinating: "While enjoying the spectacle of the play of innumerable glow-worms in the north of Italy on a warm summer evening in the month of June, one often has the illusion of an unusually swift flying and fluttering on the part of the tiny

creatures. Now there is a glow here, now there . . . The illusion arises in that one notes the contemporaneous glowing of many worms at consecutive moments of time, but interprets it as the movement of one and the same creature . . . The illusion is conditioned—speaking psychologically—in that several light sensations are unjustifiedly correlated with one and the same pseudo-substance."[46] Here we have a process of unification posited as a function of a non-sensory component as in the case of Woodworth's example. But let us note: Schultze speaks in the following paragraph of this perceptual process as being explicable only by means of a conceptual process occurring without being noticed and immediately thereafter he says that "it is probable that thus also the processes of syntactic correlation will prove to be highly complex, but fully automatized, conceptual processes".

In connection with these citations let us cite also a passage of Stumpf where he, too, appears to touch upon this problem. In a foot-note he defines his *Funktion* of awareness as that *Funktion* through which parts or relations are precipitated out of the chaos of phenomena.[57] Thereupon he says: "To be sure, there usually goes with it an instinctive positing of the part noted, and later there is often also a conceptual judgment concerning the presence of the part or of the relation." It is hardly fair to seek to interpret a position on so slight indications as may be found in a foot-note. Yet even a foot-note must mean something and indicate some direction of thought. We ask: What may be the significance of this "instinctive positing", *instinktives Setzen,* in this connection? Is it through this instinctive positing that the part-content, the phenomena noted, come to "stand over against us"? If so, then is there not, even in the phenomena something of the nature of an instinctive *Funktion* tucked away? And what is to be the meaning that we are to attach to the term instinctive? If instinctive, then it is essentially innate, but innate what? Is it an innate tendency to action?—We see that we are here coming into close quarters with certain "genetic" considerations that Stumpf so decidedly deprecates; yet by postulating this instinctive act, which later may be replaced or accompanied by a conceptual

judgment, he has certainly touched upon the problem of the relation of instinctive activity to conscious activity, and thus comes well within the domain of the biological aspects of consciousness.

We have then, here, five contemporary writers who touch upon the problem of objectivity, of the thingliness of things, Locke's problem of the idea of substance "which we neither have nor can have by sensation or reflection", yet concerning which he is constrained to note that we "accustom ourselves to suppose some *substratum* wherein they (the simple ideas of sense) do subsist, from which they do result, which therefore we call substance".[39] On the other hand we have Titchener who would deny that this is a problem for psychology at all.[95]

The function that is singled out by all these writers, however, is obvious enough. We awake from sleep in response to a sudden stimulus with a "what's that"! The psychological problem is: How is the "that" given us? Not in terms always of the "qualities" of the stimulus, for we may have in the situation a vivid "that-consciousness", these writers might say, long before the "whatness" of the "that" has been determined in the coming to consciousness of certain attributive aspects giving meaning to the stimulus. We "know" that "something was there", but it may take some time before the stimulation comes to consciousness in terms of certain sensory "attributes", or some other meaning. It is the "something" of Moore, the "pseudo-substance" of Schultze, the *Intention* of Bühler, the "thing" of Woodworth. All of these writers make the thatness a new structural category. But what would Stumpf do with our instance? Would the "instinctive positing" be precipitated into consciousness before the phenomena in terms of which the stimulus would be characterized, have themselves loomed up? And would this precipitating of an instinctive activity into consciousness then partake essentially of the nature of a "conceptual judgment concerning the presence" of some stimulus?[57] And would it be essentially in situations like that of our illustration that the instinctive positing, the thatness, would come to consciousness? Questions such as these bring us within close range of the functional doctrine of

perception. The "thatness is a function of the co-operation of many organic conditions. This functioning factor might be a typical attitude of the organism, as in the instinctive attention response. The consciousness that arises in this situation then is that of attentive straining after a "that".

Here lies the significance of such characterizations as that given by Colvin: "An object is, in the last analysis, constituted by a set of definite and consistent reactions."[24] "Thatness" in the case of the percept, would be referable to the influence of the "instinctive" innate reaction characteristic of responses to external stimulation. This factor in the total overt response becomes the basis for the attitude that contributes toward the awareness of the total situation as a reaction to an object present to sense.

While one can readily appreciate the dislike that certain psychologists express for the indiscriminate intermingling of conscious functions and motor functions, their legitimate desire to keep conceptual categories nicely apart should not lead them to overlook the fact that motor functions and conscious functions are closely interrelated. A stimulus that is making for expression in terms of one of several habits, insofar as it comes to consciousness at all, is experienced differently than when it calls out another habitual response. So, too, more narrowly, on the side of attitudes, the perceptual attitude may be felt as being distinctly different from the ideational. If then the thatness of the mental object be a function of the instinctive attitude with which the organism responds to the stimulation, it would appear that the standing over against us that, according to Stumpf, is manifested by the phenomena, is not a characteristic that is inherent in them, and that is, as it were, written across their face, but rather one that accrues to the sensory experience in the calling out of a certain attitude. The same would hold of the "imaginal" aspect of experience. Analytically sensations and images are not bare, simple, ultimate "thats" in their own right, but rather does their "thatness"—whatever may be the case with their "whatness"—arise as a result of the calling out of a definite type of functional response, originally instinctive, as Stumpf

supposes, but later singled out by consciousness and at times, under certain conditions, as in the experience of correcting illusions and hallucinations, playing a part in conscious activity and control.

The *Intention* of Bühler, the thing-quality of Woodworth, the pseudo-substance of Schultze, the "something" of Moore,—all these, then, may become distinguishable aspects of experience, accruing to experience through the functioning of certain definite attitudes. On the side of overt motor attitudes we have genetically the instinctive attention response, later develop the differentiated attitudes which might be referred to as the perceptual, the imaginal, the conceptual, yes, the interrogative even. The psychical "correlates" of these are the different feelings of "thatness", the "reality" of the object of perception, the "ideality" of the object "merely thought of." The peculiar form of objectivity, of thatness, which accrues to a "content", depends upon which one of these attitudes is functioning at the time. It may eventually be found that certain forms of mental malady are to be referred to the abnormal functioning of some of these attitudes; that *folie de dout,* for instance, is to be characterized as the tendency of a particular one of these attitudes to be set off under conditions in which in normal persons other responses are habitually called out; furthermore, that the degree of suggestibility may depend upon the facility with which one of these attitudes may be replaced by another.

Here we would appear to have the *Funktion* of Stumpf, the attitudes of Judd and Titchener functioning in such a way as to bring about a definite type of modification of consciousness,—these modifications being the different modes of thatness that arise in experience. We might be reminded in this connection of Stout's modes of being conscious of an object,[47] of Brentono,[7] and of Colvin's doctrine of attitudes as indicated in his book on *The Learning Process.*[25]

And now, what shall we do with these attitudes? Shall we follow Titchener and Colvin in their method of dealing with them, i.e., analyze them into "sensations", or shall we follow Stumpf in

denying that the *Funktion* can thus be stated adequately in terms of sensory ultimates. Or shall we go a little farther in another direction with Bühler, Ogden, Woodworth, Schultze, and others, and proceed to hypostatize the objectivity, thingliness, *Intention*, that accrue to experience as a result of the functioning of these various attitudes, and make of them structural elements?

Let us revert for a moment to Stumpf's position. While the *Funktion* was for him "immediately given" it was, nevertheless, not a content category like his phenomena. And its correlate, the *Gebilde*, likewise was not a new content. Now if the "conceptual judgment" concerning the "presence or existence of the part noted", of which Stumpf speaks, be the correlate of a *Funktion*, then it is not to be conceived of, in terms of Stumpf's system, as a new content but as a *Gebilde*.. And furthermore, if we are justified in interpreting the instinctive positing of which Stumpf speaks, as the primitive unconscious *Funktion* that is genetically basic to the overt conscious judgment, then the "thatness" arising in unreflective experience, the unquestioning acceptance of the reality of the "presentation", insofar as it constitutes this presentation a "that" which stands over against us, also cannot be conceived of as a new "content". Thus the various modes of objectivity would not be, for Stumpf, new structural categories of static ultimates. If we have been at all successful in our identification of the *Funktion* of Stumpf with the "attitude" of certain American psychologists, then we believe, that at least in this he is to be identified more narrowly with the American functionalists in that he does not favor the hypostization of certain modifications accruing to consciousness as a result of the operation of the *Funktionen*. And, we believe that the various modes of objective reference will eventually be traced back to the functioning of certain typical attitudes or *Funktionen*. To hypostatize these "feelings" of thingliness, of objectivity, as independent structural "non-sensory elements", will result in the end in a psychology as disjointed as the structural sensationalism which those who proceed thus are seeking to overcome. To add another structural element to already existing structural elements does not overcome the difficulty.

XXXII

It appears, then, that the "sensation" stands over against us, can be "inspected", because in a complex activity it has been placed over against us,—our attitude constitutes it an object. Yet here lies a subtle difficulty that arises in connection with Locke's problem. For the normal perceptual consciousness the "qualities" and the "objectivity" are given in a unity. There is no question of "is it or is it not an object?" in the thousand and one perceptual experiences of the day. It is only on rare occasions that the mind distinguishes twixt "qualities" and "thingliness." The various modes of objectivity, perceptual, imaginal, etc., need but seldom be abstracted and reflected upon, unless, for experimental purposes, or mayhap when we have become subjects in need of a psychotherapist. Yet, nevertheless, in the normal human individual they do eventually come to be distinguished, and the psychologist may ultimately discover the machinery of the overt judgment mentioned by Stumpf, getting his lead from such experiences as that in which we wittingly "try on" various attitudes in concrete situations where conditions are such as to favor ambiguity. Locke, however, did not follow this trail else he would have come upon the attitudes or *Funktionen* underlying the various forms of objectivity. He chose to follow another and thus came to postulate, as does Bühler after him, an "idea of substance" that is derived neither from sensation nor reflection. His method appears to have grown out of another mode of pre-scientific procedure which we have already had occasion to mention in another connection, viz., the method by which not the attitude is abstracted, but the type of objectivity that is experienced through the functioning of various attitudes. In other words he abstracted not the *Funktion,* but the "correlate" of it which in Bühler's scheme becomes pure thought: *Intention.* The method is that of the child that delights in staring fixedly as some word upon the page in order to get the strangely mystical experience of "losing" its meaning; then singling out a letter and losing its meaning, and after some time spent in losing meaning after

meaning, he finally emerges once more to take up his normal activities, yet feeling the awe of the little mystic who says:

> "I know something more
>   Than just a moment ago,
> I know something more—
>   I wonder what I know?"

Our thought psychologist believes that he, too, "knows something more", viz: that "thatness", "objectivity", the feeling of "something", may continue to be "given" even after all the sensory "qualities" have been abstracted from the "object". These qualities, he finds, may change, yet the object may remain the same. Having abstracted from all the sensory qualities he finds that the "thatness" persists. He now attempts to account for this "thatness", and finds that since he believes that all that is "given" him is the qualities, he must assume a "substance" in which they inhere in order to constitute them a "thing". Hence Locke's "vague idea" of substance, gotten neither through sensation nor reflection, his only sources of knowledge. From the point of view gradually developing in contemporary psychology Locke is perhaps building better than he knows when he attributes this vague idea to "custom". We need not accept the implication that genetically there are first the "qualities" and then the gradual development of the "objects" through "custom"; but say, rather, that the thingliness is the function of the instinctive attitude that the biological form assumes with reference to the stimulus. Thus the "custom" in question becomes not an individual but a racial habit.*

But having succeeded in abstracting the "qualities" from the "that", we must not commit the error that James repeatedly warns us against of assuming that the original unanalyzed experience is the sum of these two hypostatized abstractions: The one "sensory", the other "non-sensory" or pure thought. Bühler and the other thought psychologists who make the *Intention* a

* Our Lockeian, however, might balk at this point, for fear lest he come dangerously near the bugaboo of "innate ideas."

static non-sensory element are using a method of procedure identical with that of Locke. With such a procedure the "qualities", the simple "ideas of sensation", acquire a thatness of their own as sensations, and this thatness is other than that of the unanalyzed experience of normal perception. The "qualities" are some of the meanings that may be successively shelled off from the *Ding-an-sich*. In making the sensations elements they retain this conceptual thatness of quality and only so can they be characterized as "standing over against us" with a thatness of their own, yet independent of the thatness of the substrate in which they are supposed to inhere.—We have here a suggestion toward a statement in psychological terms of the problem: How does the mind know its object; which psychological statement might possibly be found to point the way toward a workable metaphysical statement.

Bühler and others are quite consistent in carrying out the Lockeian analysis to its logical conclusion. If our aim is to analyze the percept into its "elements", and we are satisfied to make of the qualities sensory elements, then it is only just to make the "vague idea of substance" another element. Yet in proceeding thus the "sensationalist" and those who find the "imageless thought" are alike following the method that, whether it be admitted or not, grows out of a conception of consciousness as an entity of which the "sensations" constitute a part. Having conceptually created the sensation entities, we question the "power" of the "sensations" to fuse and to constitute an object.[105] But to proceed forthwith to create another sort of entity that is to fuse them, as Woodworth does, for instance, leaves us still with a disjointed experience. The difficulty is that the "mere sensation" is for them an entity already, but not the same entity as the colored sounding hard object that compels our attention. But to create another non-sensory "thatness" gives us not the fused, unified, mental experience of normal perception, but the state of mind of the philosopher, facing the problem of the relation of the "qualities" to the *Ding-an-sich*.

It would appear, therefore, that Bühler's *Intention* is, as he

claims, truly a distinguishable characteristic of every actual conscious experience. We saw that the type of *Intention,* of thatness, may be very various indeed. But if it should be found that the whole of conscious life always divulges this "ever-present" aspect of objective reference, for which Brentano[8] contended nearly forty years ago, then Bühler is doing contemporary psychology good service in insisting that investigation be directed upon it. Granting this universality for the nonce, with what current concepts of psychology will it be found to have elements in common? Are we lead to correlate the "thatnesses" with the focus or foci of consciousness? Does the focusing constitute objectivity? Are the various "thats" felt as different by reason of the functioning of different attitudes within the total activity? In how far are these different "feels" of thatness, which we are now discovering, comparable with the "modes of being conscious of an object" of Stout?

Chiefest of all the service that has been rendered us by the "thought psychologists", among whom Bühler, beyond a doubt, has been the most active and heroic, is this: It forces us to face the question as to what constitutes our concept of sensation and its relation to the other psychological categories. Not the question of the presence of "non-sensory elements" is first to be decided, but rather must the psychologist first consider the question of the meaning of "sensory" and of "element".

XXXIII

If the "thatness" of the sensation accrues to it by reason of the functioning of a specific attitude, *Funktion,* or *Bewusstseinslage,* then it must be stated not as a peculiarly inherent characteristic but as one that is derived. And what about the other "attributes", —are they inseparable, ever-present aspects of an ultimate, "given" sensation element?

We have cited those who would demur at the rigidity of the interpretation of the concept that, as was pointed out, continues

to bear the marks of its origin in connection with a conception of the nature of mind that makes sensation a relatively static, "meaningless" factor in experience. We have cited Külpe as proclaiming the possibility of abstract *Vorstellungen* in "actual consciousness", and relegating the sensation of the inseparable attributes to a nether-world of "psychic reality". A suspicion is arising whether sensation is to be adequately stated in terms of the old definition. May it not be that the sensory attributes may themselves be found to be not ultimate, but developments in a complex process? Like all individual meanings the sensory *qualés* as experienced are developments in an organic system of meanings, a growth within the individual's experience.

Külpe points out that the form or pattern of the sensory stimulation may be reacted to by consciousness to the exclusion of other sensory attributes. Angell points out that on the side of the development of the individual mind grossly different forms are reacted some time before qualitative discriminations are made. This would be the genetic statement of that which Külpe notes. If we consider the conscious experiences involved in these reactions noted by Külpe and Angell as sensory, then the sensationalist must admit that it here possesses a character very different from that of those other experiences into which the qualitative distinctions have entered in.

So far as consciousness is concerned, the attributes come into being within the actual experience of the individual only at the moment that a discriminative reaction occurs.

Human faces, for most of us, are perhaps among the most interesting of objects. The mobility of the nostrils, the fine lines about the eyes, the sensitiveness of the mouth, the depth of the eyes,—all these are the basis of the subtle differences in our reactions with reference to our fellows. And all this is mediated by visual stimulation. We may have thought that we were fairly familiar with these impressions. Yet on a day we wander into a portrait gallery, perhaps it is our first time there, and as we look at the pictures a sense of strangeness comes over us. These are not like the faces of real life—never were there such

deep shadows on any human face. We go out upon the street and peer into the faces of the men and women that pass us—yes, the painter was right. And henceforth the faces about us come to have for us an added richness of meaning. Now if color tint be accredited a sensation attribute, then the doctrine of the sensation element that comes into being always and only with all its attributes must make its peace with the facts. Schultze points out that the artist sees various tints and hues where the layman sees naught but white snow.[45] In Pillsbury's words the layman might be said to experience the "type".[44] Again, in glancing at a brass vase one does not always perceive the distribution of light and shade, but as in the case of the human face percept, mentioned above, the color of the surface is often perceived as "all of a piece".

Another case in point: The teacher of introductory psychology has every opportunity to study the process by which the attribute of "saturation" comes home to the consciousness of his students. He will get from them the surprised remark that this has changed their whole consciousness of color. Others have vaguely "felt" it, but it had not been quite able to come clearly to consciousness. Others never can "see" it. Surely Külpe's distinction between conscious actuality and psychic reality is a most useful one at this point for taking cognizance of the facts of difference that we note here. If the "real sensations" are the same throughout, well and good—let those who need it take comfort in the thought—but the actual consciousness, the consciousness of our world of thinking[43] and aiming and striving, is very different not only on the side of mere character but also on the side of control, after the discrimination has been made. The newly discovered attribute is definitely a function of the complex attitude into which we sought to bring our student by means of our similes and suggestions in figurative speech. And having succeeded in bringing the meaning home to him means just this: the enrichment of his experience by this new mode of conscious reaction upon visual stimuli; and the method by which it is acquired is essentially the same as that by which we are led to acquire any other new con-

scious reaction—as when one attempts to put another into that attitude which will make possible for him the noting of the hidden figures in the ambiguous drawing.

Now, either this attribute of saturation was always a part of the student's actual conscious experience and that of color tint always involved in the perception of human faces—in which case it must be shown how these attributes were involved, whether marginally or "sub-consciously" or as neural activity merely; or else they come into existence as *conscious experience* in that first discriminative reaction with reference to them.

If we take this latter point of view, the "sensation" as a fact of consciousness must be stated as a development within the individual's experience which is enriched only gradually by distinctions of sensory attributes. Genetic considerations lead us to note the necessity of discarding the definition of sensation as a sum of fixed inseparable "attributes", a definition that has been taken over unquestioned from certain philosophical systems. Sensory consciousness is no simpler than, but equally as complex as, any other type of consciousness and the laws of its coming and going are the same as those for the rise of any other form.

If we take even the simplest of our sensory experiences, we shall find always that it can be adequately stated not in terms of a bare "what", but that it involves a complex reactive process to make it what it "is". Schultze tells us that the "whiteness" of the perception of the snow may be not sensory but "conceptual".[45] The "thatness" of the quality also is, as we have seen, not as Stumpf and Stout[48] would have it, a matter of inherent objectivity, but as our analysis leads us to believe, likewise a function of the total situation in terms of which the sensory experience, as Angell puts it, can alone be described. We have noted that the process by which an "attribute" of visual experience comes to consciousness for the first time, is a complex process, and our account is very like that which Moore gives of the hypothetical rise of one of his non-sensory categories.[42a] Our similes, our circumscriptions, our figurative speech, were essential factors in creating a situation for our student that made possible for him

the experience of the saturation aspect. To use the terms of Moore, what we really created was an imageless category to which the data of sense experience are assimilated.[41] For it is these new attitudes that constitutes essentially one of the non-sensory elements of the thought psychologists. When therefore the thought psychologist postulates a "new element" he is attempting to overcome an inherent difficulty that he is coming to feel with reference to stating consciousness in structural terms. But the difficulty does not lie as he supposes in the insufficiency of the traditional number of "ultimate" structural elements with which he has hitherto operated, but rather in the fact that he is trying to operate with "ultimate" structural elements at all.

XXXIV

If the sensations be not simple structural "ultimates" of consciousness, we return to our question: How did the conception have its rise?—For the early philosophers of the modern period the problem of knowledge was dominant. How does the mind know the world? The mental objects were given in terms of sensory qualities; they were "impressions" of the real object upon the *tabula rasa* of the mind. Permutation and combination of the "simple ideas" gained in sense impression, give rise to all other "ideas of sensation." Then there are certain phases within the process of permutation and combination which may momentarily become "objects" in themselves and we have as a result those other "ideas of reflection", ideas of conscious activities. But always the "ideas of sensation" are fundamental.

Thus sensation was an ultimate genetic category. Through this gateway all knowledge comes to us, and thus the philosopher was able to lay low the hobgoblin of "innate ideas". To be able to proclaim that there are no "innate ideas", but that all knowledge rests upon sensation, gave tremendous satisfaction to the empiricists. Soon the genetic category, however, developed into a content category. For Hume "qualities" are the only elements of

cognitive consciousness. The subsequent gradual development of this conception of sensation as a structural category is an important item in the history of empirical psychology.

Under the influence of physiological considerations the "impressions" of Locke and Hume cease to partake of the nature of "copy" of external objects and become essentially the conscious correlate of the physical stimulation of end organs, or of the revival of cerebral activity underlying past sensory experience. Yet the influence of the "impression" theory still continues to be felt in that the "sensation" and "image" is a real "item" of the experience toward which the sensory stimulation contributes. But another determining factor is that of Kantian philosophy. In so far as we have in our contemporary definition of sensation the item of "meaninglessness",[42 80] we note the influence of idealistic philosophy with its distinction twixt sense and understanding.*

Arriving at the conception of Wundt and Titchener we find that the sensation and image element is not the total mass of sensory impression, or its revived image, as represented by the "ideas" of Locke. These latter are conceived as "compounds" of "complexes" that are to be analysed into "elements". These elements then come to be minutely studied in isolation and thus we have attained to that massive body of knowledge on the psychology of the senses. We may say that the sensationalists have been fairly consistent in their structuralistic procedure. Actuated as they are by the motive to trace consciousness back to its source in sense, they attempt to *analyze* all cognitive consciousness into component "sensations" and "images." Whatever we may say concerning the nature of these as "ultimates", the structural analysis may be regarded as legitimate only in so far as we always bear in mind that when we say that an experience is analysable into these and these "sensations", we can thereby mean only that the sensory stimulation now mediating these "sensations," and the cortical excitations underlying the "images" now in consciousness under conditions of introspective analysis,—that this sensory stimulation and these cortical excitations were functioning as a

* See above, Section XXIII.

unity in the experience that was held up for analysis, in such a way as to contribute toward that peculiar experience of the conscious attitude of doubt, or the unified percept, in the actual consciousness of just a moment ago. In so far as this position is taken, the analytic psychologist, until he finds good grounds for abandoning this method, is justified in "dissecting" actual consciousness into "sensations." And all the while he can do justice to the fact that actual consciousness appears often to have but little in common with the sensation of the inseparable attributes. But the moment he maintains that the attitude or percept *is* in any way the sum of these sensations of the inseparable attributes as actual conscious "processes," one must enter demurer. For it would pledge us to the doctrine of static conscious elements,—static, no matter how much we may protest that they are "processes". Actual consciousness cannot be thus conceived.

Hume, however, was the point of departure not only for empirical sensationalism, but also, through Kant, whom he had awakened from "dogmatic slumber," for quite another movement in philosophy, that in its turn has influenced psychology. The "sense impressions" of Hume became the phenomena of Kant. Over against these was the Reason that manipulated the phenomena. The "real objects" of Locke became for Kant the noumena, that underlie the phenomena. Kant, to be sure, despaired of the possibility of an empirical psychology, but his influence is clearly manifest in the psychological trend that we are considering here. It is the activity character of consciousness that occupies attention to-day. That this very aspect that Kant despaired of seeing attacked by empirical methods, is also the very one that the thought psychologists are grappling with, is evidenced by Bühler's remark after the following citation from Kant: "This schematizing on the part of our intellect is an art hidden in the depths of the human soul; and it is hardly probable that we shall ever discover and lay bare this knack of nature." Hereupon Bühler says: "It is evident that Kant has allowed empirical considerations to enter in, and if we continue them systematically, then we need not despair of our problem; it may

prove difficult, but we certainly shall discover and lay bare these 'knacks' (of nature)."[16] For Kant, we saw, there were two fundamental categories: sense and understanding. Through sense the phenomena are given: through the understanding, the meanings. How these came into being and how they functioned, Kant despaired of ever discovering. Yet after him, we note his influence in certain developments in contemporary psychology. The *Funktionen* of Stumpf, the "acts" of Brentano, and the conceptual categories and meanings that arise as a result of the operations of these, still bear the marks of their derivation, historically, from Kant's system.

In so far as the thought psychologist adheres to the Kantian ordering of the categories, we run little danger of developing a new structural "thought element." "Thought," for Kant, is an activity, not a static given like the phenomena. In Stumpf, for the most part, there is logical consistency in this respect. But the psychologist with this philosophical bias has been trained in the analytic structural psychology of the sensationalists. Now the phenomenal mind of the sensationalist, no matter how much he may protest to the contrary, is a somewhat that is to be dissected into its elements. He has learned to note some of these elements and has discovered much about their characteristics, and it is in terms of these characteristics that he has defined them. Thus the realm of phenomena has been minutely explored and the results of the exploration taken up into the definition of the elements. Yet all the while, the mind of the thought psychologist is occupied with his heart's desire, to understand the *working* of the actual mind. But Kant himself had despaired of ever knowing that, except by its results, and so one continues to labor in the vineyard of the sensationalist. Analyse, analyse, analyse; state this, that, and the other formation, as colligation, as fusion, or as assimilation, if you will; but above all things, analyse them. So, like little Johannes of the Quest, they kept at it. But, as with Johannes, a suspicion arises in them that what they are finding under these conditions will not lead them to their heart's desire: the understanding of the working of actual consciousness.

They occasionally make faint attempts at piecing together the phenomenal elements, to compare them with the original experience,—but that is a violation of the prescribed method.

Yet finally they take courage, and rise in open revolt. The spell is broken, and they secede. They call the domain of the master in whose vineyard they toiled in weary captivity, the realm of Psychic Reality. Their own territory they call Conscious Actuality. And now they set to work. But what will they do for a method?

Trained so long in the school of the sensationalist, it is small wonder that they take over much the same system of habits, and much of the conceptual machinery developed there. The sensationalist takes his "given" experience, the percept or the attitude, and holds it up; he now has a new mental state in which he finds sensations of special sense, kinaesthesis, etc. He may conclude that the sensory stimulations and cortical excitations that now mediate these sensations and images, when functioning in the original state were to be correlated *in part* with the consciousness of percept or attitude. The tendency, we saw, is often to go a step farther, however, and to commit the fallacy of saying that not only this sensory stimulation and cortical excitation, but rather also the elements of the inseparable attributes presumably now mediated by this neural activity, were also present. We forget that the "sensation" and "image" are partly a function of the neural activity of a different cortical system than was involved in the percept that was called out a moment ago in response to the same physical stimulation, which percept we held up and "analysed."

Now some of the thought psychologists are just as prone to fall into this error as is the sensationalist. Following accepted methods they take, say, a perceptual experience, and ostensibly try to discover any sensations or images that answer the structural sensationalist's definition of the "sensation element." Thus they continue to regard actual consciousness as being capable of a static, structuralistic analysis. And now they do what they did not dare do under the old régime: They take, or try to take, the

"sensations" and "images" that were analysed out and compare them with the feel of the original experience or one that they believe to be like it. The result is that they discover that over and above the "sensations" and "images" there is "something more", and that is the "non-sensory" element. The method by which this is obtained is interesting: Take three persons, segregate them, stimulate each with a tonal stimulus. As a result you must say that there were three sensations experienced. But put the three sensations into the same consciousness and you have not only these three sensations but something over and above them: you have a clang or a chord. Again, take three lines, you can experience them separately in certain definite positions and you have simply "lines"; but experience them together, in the same positions as before, and you have not only the three lines but over and above these: the "triangle." The form-quality is the "non-sensory" component. Or again: given a melody in a certain key; then the same melody in another key. There is a common "element." So far as "sensations" are concerned the two experiences are wholly unlike. But they have something in common: the "form." This, then, must be something over and above the sensations.

Where the sensationalist, having analysed, ostensibly stopped short, the thought psychologist does not stop, but approaches his "original experience" of "actual consciousness" and compares the products of his analysis with the original. He notes a difference. This difference is postulated as "thought element."

Had the first philosopher of this attitude of mind—shall we call him Plato?—stopped short where the sensationalist stops, and then had no one after him dared to depart from his example, we should to-day have no "thought element" to combat. But it so happens that this first one did do the wicked deed, and since then there are those who follow his example and attempt to tally their analysis by some form of synthesis or reconstruction, and always there is "something more." As to method, the sensationalist analyses and seeks to explain in terms of "body-processes"; the other analyses and then attempts some form of

reconstruction. And as a result of his attempt at reconstruction he discovers that somehow, in the first analysis he overlooked an "element". But having found it, he henceforth knows enough to look for it at once and sure enough, it is always there.

Thus far this factor of form, pattern, plan, meaning, has been considered only as it is supposed to appear as a "funded content" in connection with other, sensory, elements. But one day our thought psychologist comes along and finds that the sensations of the inseparable attributes are not always discoverable, yet the lamp of thought burns bright as ever. Sensations and images sometimes occur, but they are as "sparks struck off from thought in its progress rather than thought itself"[100] and they "hinder rather than further thought." All that there is in consciousness is "thought," which is quite unlike the meaningless sensation or image of the inseparable attributes as defined by psychology as a science. Actual consciousness, he concludes, may therefore operate without "sensory" material. No longer is there need of stopping to analyse in order to find the sensations and images; when they do appear they are known for what they are, and then, for the most part, they are a nuisance.

Thus our thought psychologist, when he turns from consciousness as sensation-mongering and image-mongering, to consciousness as problem-solving, decides that consciousness is not made of "image-stuff" but is essentially of the nature of "thought-stuff."

This then, is the account of the rise of the "sensation element" and of the "thought element." Both of them are due to a conception of consciousness as a somewhat that is capable of static analysis, a somewhat that is constituted of part contents which are to be described "as they are, existentially." That the sensationalist, however, discovers but one class of cognitive elements whilst the thought psychologist discovers two, is due, as we have seen, to a difference in their methods of procedure.

XXXV

Our thought psychologist concludes that there are two modes of representation: one imaginal; the other *reingedanklich,* pure thought. Bühler tells us that he can think a specific blue object in two definite ways, first in terms of a visual image of the blue, and then in terms of a purely *unanschaulicher Gedanke,* and that the second is just as definite as the first.[13]—Now if an observer should be found who only rarely has visual imagery and whose auditory imagery is dependent in large measure upon the motor factor involving the vocal organs as a provocative,—the visual image being a rarely occurring phenomenon and evincing a profound emotional response owing to its great unusualness and the auditory imagery being bound up with the motor factor so that it is seldom a free rising image, (and yet with all this the person takes a keen delight in music and art presentations, but nevertheless finds it well-nigh impossible to recall in terms of auditory or visual representation)—if such an observer should be found, we would verily have a case of one who must and does refer to the objects of his world in the manner that Bühler subsumes under the head of imageless thought. But if such a one should maintain that his whole conscious life is shot through with kinaesthetic experience—that every attempt to introspect precipitates such experience—that the meaning as kinaesthetic imagery often comes just as near to the surface of the stream of thought as the meaning of auditory in an auditory idea,—then for such a one it could not be maintained that his thought was any more non-sensory or imageless than is the auditory idea. It is no more "mere" kinaesthesis than is the auditory idea merely auditory imagery, for the moment it becames kinaesthesis the "thought" is gone.

Our observer, we maintained, is almost wholly lacking in visual ideas, and auditory ideas are supported as it were by the motor factor of the vocal organs. But over and above this type of experience involving auditory factors, our observer maintains that there are others of another type, viz., just such cases as that of Bühler's example of imageless thought that was quite without "sensory" factors. There are certain definite

spheres of thought, certain "areas" of experience that involve this type of activity. Indeed, our observer maintains that this mode of thinking is in certain situations of his daily life a most common occurrence during long stretches of time. In sooth, he would be characterized by the thought psychologist as an extreme example of "imageless" thinker. . . In human intercourse persons of this type experience much of what Lipps and his school appear to subsume under the term of *Einfühling*. Their attention does not appear to apprehend primarily the visual presentation of another person before them, but rather his "very soul". They come away with little knowledge of the person's outer appearance, but with a lively appreciation of his "inner life". The writer has taken some pains in questioning this type and he finds that many are surprised that not every one "knows" others in the same manner as they do. The other person is experienced "from the inside", as it were. There is a precipitant grasping of another's meaning and a reaction to it before the other has uttered a word. Such anticipation may at times approach close to the uncanny. To some, this revelation of the other's personality precipitates often a moral problem: is it right to look so deeply into the living reality of another's being, for the very fibre of the other's moral personality seems to be felt. The tendency "to put one's self in the other's place" is singularly marked in these persons.

Now such a person, though he can summon no visual image of some particular blue under normal conditions, can still adequately refer to it in his thinking. He can bear witness to the absence from the experience of all "irrelevant images" with their inseparable attributes, and to the presence of all that Bühler claims for his "non-sensory" thought. There is definitely "whatness" and "thatness",—*Wasbestimmtheit,* and *Intention.* Indeed the thatness aspect, the *Intention,* is often a most prominent aspect of the experience.

In the case of such experiences like that of Bühler's example, the attempt to introspect reveals definitely a fleeting consciousness of motor attitudes that, he believes, as attitudes functioned in

the thought by contributing especially towards the reference aspect, the *Intention*. Whenever such attempts are made to hold up a memory experience in which the awareness of another's personality is central, the liveliest of kinaesthetic imagery is precipitated. There is remarked an absence of imagery of the special senses, but there is vivid imagery of posture, of holding the head, of finer "feels" of characteristic movements of muscles of the face, especially of the lips and eyelids.

When, in Külpe's experiments, in the case of abstraction of pattern or form from the visual presentation in actual consciousness, the person is said to *see* actually only the form, and that quality is abstracted from, we have a case that may be placed alongside of the getting of the "meaning" of another's "personality" as a part of the immediate percept—as it were. Our observer, too, often can tell you nothing concerning the visual appearance of the person, there is often no immediate memory of the color of clothes, hair and eyes—in short no visual "image" to give these details. And yet our observer has the knack of pointing out aspects of character in another person that others have discovered only after long acquaintanceship. Our observer when attempting to hold up such an experience notes *kinaesthetic imagery* (which is not to be confused with visual images of movement or kinaesthetic sensations), and definitely emotional factors.

On the side of mechanism, such a person cannot be "set" for the sensory aspect of visual experience at all, but the path of discharge appears to be mainly in a direct route away from the visual cortex to the kinaesthetic and motor areas; this would appear to be the case especially when at some moments of experience our observer almost "forgets" that he is not the other, but experiences with him, and is often actually "embarrassed" with the other, in a situation. It is small wonder that this mode of consciousness should appear to be more nearly allied to the nature of "thought stuff" and seemingly more intimately "psychical" than imagery of special sense. It appears to be peculiarly of the essence of "meaning". And when Woodworth[105] main-

tains that the sensations are not the "meaning", that they rather "call out" the meaning, our observer believes that he knows what Woodworth is getting at. The visual image, in the case of individuals in whom it is common, may be as "irrelevant" to the meaning as is the visual "sensation" in the case of the perception of another's "inner life" on the part of our observer. The "sensations" for Woodworth, call out the "meaning". Now it appears that in so far as consciousness is concerned, the visual characteristics of the person are not noted by our observer at all; all that is "there" in consciousness is the "meaning". For our observer on the side of *recall,* however, the visual factor drops out almost completely excepting in so far as the "feel" of its having been originally a visual experience may be present as an "attitude" (which is not a subtle, vague thing for the person of this type, but often a very actual conscious experience, of the kinaesthetic type). Woodworth's observer would not appear to be of the extreme type that gets the "meaning" alone when *perceiving* a person, and in whose consciousness the visual aspect is abstracted from often almost completely. And on the side of recall, also, it may be found that variations occur. Woodworth's observer, when he "thinks" of the person, might get the visual aspect along with the meaning; our observer maintains that the visual image does not enter in. There may be also another difference on the side of recall. The whole organization of the extreme kinaesthetic type appears to be that to "call up" an experience, a thought complex, the initial process is always a certain "attitude" that then "brings up" the "thought"; it may well be that for Woolworth's observer the recall is *via* the *image* of special sense which is necessary to call up the "meaning."

Bühler appears to regard all thought which he does not believe to be re-presentative but representative, as "imageless," and all "imagery" that he does not believe to be re-presentative as "irrelevant."[27] Yet it may well be that the kinaesthetic type of activity is just as re-presentative for some persons as the visual is for others. And when Bühler does note just such phases of experience that the introspective purpose occasionally loses from

the relations in which it is occurring, he shows the operation of the fallacy that we pointed out in our discussion of method. He says: "And if, in the case of such experiences which might be expressed by such words as 'but' or 'or' or 'nevertheless', there is a feeling as if I were going from something at my right to something at my left, or from something in front of me to something back of me—one surely would not wish in all seriousness to generalize upon this fact of experience in the form of a conclusion that the real conscious correlate of these ideational continuities is to be sought in these sensory elements."[10] Bühler here proceeds as do all those who conceive consciousness as a "substance" to be statically analysed. He assumes that sensations are "real items" of experience, entities, that may float upon the surface of consciousness, but do not constitute its essense. Its essence is another entity: *Gedanken.*

Let us bear in mind the point that we arrived at as a result of our inquiry: that the mistake of both the analytic sensationalist and that of the analytic thought psychologist is often that they forget that the conscious moment is as much a function of the "modified cortex" as of the specific character of the inducing stimulus. If, however, a conscious experience is to be characterized as "sensory" on the score that when the shift to the introspective purpose takes place, we find certain "sensations" and "images" present, and if we proceed on the hypothesis that we may infer that a part of the nervous mechanism that is supposed to be involved in this activity of analytic experience was functioning also in the interrupted experience—if such an experience of "actual" normally functioning consciousness is to be characterized as "sensory",—on this score, then, those very aspects of experience that the thought psychologist characterizes as essentially "non-sensory", must be put in the same category as the seemingly more patently sensory re-presentative experiences harking back to the functioning of the organs of special sense.

XXXVI

When Titchener characterizes the thought psychology as being guilty of reflection,[93] it might legitimately retort that it is no more and no less reflective than structural sensationalism.

The various forms of mental activity, imagination, perception, judgment, etc., as mental activities are supposed to have a different "feel", yet the unreflective person may never have become aware of these subtle differences; and only upon reflection does he become conscious that there is such a difference between the "feel" of the percept, and that of the thought, whether imaginal or "pure", still referring to the same object the moment he turns his back upon it. It is only when he begins to question the status of the two "objects" that the attitudes come to consciousness in some way. Do the "feels" of these attitudes have any sort of existence before we begin to reflect or question with regard to them? Would we have to assign to them "psychic reality" until reflection sets in and brings them into conscious actuality?

This much appears to be true: Reflection, not necessarily psychological reflection, does bring to pass the awareness of differences in the "feels" of the attitudes. It is only at those stages in individual development at which changes or conflicts in attitudes occur that we become aware of them,—that we become conscious of the difference of the "feel" of the attitudes, and this precipitation into consciousness upon further reflection may again be operative in bringing about changes in the "feel" of other attitudes.

But this process of increasing the richness of the feels of attitudes need not have its source in scientific psychological reflection, for there are precipitations of attitudes into consciousness prior to the stage of reflection. In the actual normal development of the individual sudden changes and conflicts in emotional and purposive attitudes occur, which force the distinction between them home upon him. Paul on the road to Damascus "saw a great light." This made a different man of him on the side of overt behavior; but chiefest of all let us note that it is at the

point of such sudden changes that the attitudes are themselves precipitated into consciousness, and whatever may have been their status before, they now have a conscious meaning or "feel." In this sense Paul's inner experience is just this much richer than that of the "born" saint, whose mature attitude is the product of a gradual development. This then as an example of a conflict or sudden change in attitudes precipitating both into consciousness. The earlier attitudes may have served a function within the life activity of the organism, but they were not experienced as *conscious attitudes*. So much for attitudes coming to consciousness as a result of motor conflict. Generalized: attitudes, whether instinctive or habitual, are precipitated into consciousness at the point of sudden change or of conflict between them.

Next we note that once the individual has attained to the level of reflection, it is through this reflective activity that changes are introduced into other instinctive and habitual attitudes. Inject reflective consciousness into instinctive and habitual modes of response and the activity that was unconscious, now becomes conscious and this coming to consciousness of the attitudes tends to change them, not merely by making them conscious, but in that the precipitation into consciousness tends to result in some readjustment within the co-ordinations constituting the habit or activity on the motor side. Reflection in a given case may make possible, say, the coming to consciousness of certain stimulations from the moving muscles of the legs in walking. The result is that an observer notes that we possess a "conscious walk." The *act* as overt motor occurrence is thereby changed. Among the attention attitudes, which on the physical side are perhaps the only types of acts that ought to be referred to as attitudes, we find that the development on the unreflective level has advanced far before elaborate reflection may enter in. We find various of these attitudes prevalent in subhuman reactions. "Doubt" and "belief", on our tentative hypothesis, would be originally matters of overt attitude. It is only when conflicts occur, that the attitudes gradually become *conscious attitudes,* and it is in the refinement and differentiation that results from their becoming

conscious, that we must seek the beginnings of the elaborate differentiated attitudes that are involved in the complex activities of the higher intellectual functions.

Genetically our hypothesis would see in the most elaborate scientific attitude,—the holding of judgment in abeyance, the functioning of control factors, of regulative maxims—in all these we would see a gradual development that is a result of differentiation and re-coördination of primitive instinctive attitudes that develop at first primarily in the immediate response to peripheral stimulation, but soon differentiate also along lines of ideational activity. Children's "lies" (note quotation-marks), to cite the most notorious example, are referable, on this hypothesis, to the assumed fact that differentiation between the perceptual and the ideational attitude-complexes, has not gone on apace. Now reflection hastens the process of differentiation and reorganization at the point where, through it, the instinctive or habitual attitudes are precipitated into consciousness. The thought-psychologist necessarily becomes aware of subtle distinctions in attitudes of which he never before had become conscious. Such a precipitation becomes, willy-nilly we believe, the beginning of a reorganization just as truly as the injection of consciousness introduces change into the walking.

Generalizing tentatively once more: not only does consciousness arise at the point of change or conflict in habitual attitudes, thus resulting in the precipitation of "feels" of the attitudes; but furthermore the injection of consciousness into an ongoing habitual activity through "reflection", tends to change the activity. The "acts" of the Brentano school, the *Funktionen* of Stumpf, the "attitudes" of Judd and Titchener, the *Bewusstseinslagen* of Mayer and Orth (*Bewusstseinslagen* are well defined by Bühler as a "consciousness of the thought process and especially of the turning-points in this process, in the conscious experience itself")[9]—all these might thus be said to have become definitized in the actual experience of the psychologist in the course of his reflections on the nature of thought and mental activity. Yet the beginnings of this conscious definition, leading

eventually to an hypostatization, would lie genetically in the originally instinctive attitudes as types of motor reactions within certain environmental situations. So much for the genesis and the nature of conscious attitudes. Functionally and as "felt", their character is a product of the process of organizing going on in the psycho-physical organism. Both on the side of function and of conscious "feel" they develop partly under conditions of conflict and change; partly their elaboration is due to their being caught up in the organizing activity of consciousness. . . . In other words the instinctive motor attitudes are precipitated into consciousness at the point of conflict, and on the other hand the being drawn into the organizing matrix of reflection tends to change them.

If then, the "attitude" as a conscious "feel" of activity is thus a development and not an "ultimate", what about the "sensation"? We would submit that just as an attitude becomes a conscious attitude, a feeling of activity, a conscious *Funktion,* a *Bewusstseinslage,* under conditions when there occurs a conflict or sudden change on the side of unconscious motor attitudes, and just as reflection may precipitate attitudes or habits of reacting into consciousness and thus enrich consciousness by specific "feels" of activity, of *Funktion,* or what not, so too, the enrichment of the meaning of experience as sensory may occur through the simultaneous or successive operation of stimuli calling out different conscious reactions.

If we were to construct a hypothetical world in which no light waves other than red were to act upon the retina, and if we were to be reared from childhood in this hypothetical world, we may ask ourselves what sort of a visual consciousness would the child have, and what would be his experience when suddenly a yellow is introduced into his world? What will our answer be? For our answer will reveal our notion of consciousness, of mind, or what not. We believe that a getting together on such highly hypothetical question as this, would do much toward lifting the cloud of misunderstanding now patently existing amongst the various "schools." In what sense is the redness the same before and after

the introduction of the yellow light? Was there any conscious redness at all before the noting of the difference between the two lights? Did it exist in Külpe's psychic reality before the precipitation into conscious actuality? Or will we say simply that the redness and the yellowness came into conscious existence at the same time?

We would here revert to our analysis of the behavior of meanings. If this analysis was essentially sound, we conclude that in so far as it is dependent on objective stimulation, consciousness is aroused only at the point of differences, of change in that stimulation; if the objective stimulation remains constant, consciousness tends to subside. In so far as consciousness still continues to respond to the uniform stimulus, this is conditioned by a shift in the meanings evoked by the stimulus. As Angell has pointed out we must note always new aspects, new sides, new attributes of the object if we would keep it in consciousness.[2] If this statement has any justification, then, applied to our hypothetical child, the redness would be born into consciousness only at the point of the introduction of the differing stimulation. Generalized once more: the unfolding of experience as sensory would be a gradual development; each and every new discrimination thus induced by the introduction of new forms of stimulation, would tend to change the actual conscious experience of all the related "elements."

Such a statement, if we may entertain it at all, even hypothetically, would be supplemented by another, which we have touched upon also in our earlier attempt at analysis. Not only does consciousness appear to arise at the point of change in objective stimulation and tend to subside when habituation has set in; but conversely; when reflective consciousness is injected into the sensory phase of experience, it tends to introduce changes therein. If we revert to our case of putting our student into an "attitude" that would enable him to discriminate the aspect of saturation, we have a case where reflective consciousness functions in bringing out the new aspect. The "attribute" becomes a function of the "attitude" which we create in the student by the use of similes

and what not. In other words, the process of differentiation, begun at the point of change introduced into the objective stimulation, is continued with the rise of reflection, and thus the constitution of sensory experience as conscious becomes just as much a function of "reflection", as are the attitudes. And a psychologist, interested in the activity phase of consciousness, might just as legitimately make the attitude the ultimate of experience instead of the sensation. Our analysis, however, tentative as it is, would lead us nevertheless to conclude that a more adequate grasp of the nature of human experience will be gained by a realization that neither the one nor the other is "ultimate."

XXXVII

With this, we would close our attempt at analysis of certain problems in contemporary psychology. . . . We have meandered along a devious path. . . . We have discovered bit by bit that it is verily true, as Titchener points out anew[31] that all consciousness is in continual flux. We have seen that observation of consciousness, from the point of view of a psychological purpose, inevitably introduces changes into the ongoing process. We saw also that the very changes that a psychological purpose introduces into the process, are of vital interest to the psychologist, for it is at this point that psychology may give to the life of to-day a contribution in the way of control analogous on the side of "inner" experience to the control given by the physical sciences on the side of "outer" experience. . . . The problem of the "actings" of the mind has come home to us anew, and calls for investigation. Even though the psychologist should find that they are but "another way of looking at" what is "existentially" "one and the same process", this "other way" of looking at it may bring to light new facts about the "process". Is this awareness of the "actings" contemporaneous with the awareness of the "objects"? Is it peculiarly marginal as compared with the percept, and must we therefore distinguish between the functions of the focal and

marginal consciousness? Questions such as these, along with the questions of meaning and purpose, will have to be faced squarely by a psychology that would do justice to the subtler aspects of consciousness. . . . We have seen, furthermore, that the attempt to get at consciousness "as it is, existentially" under the influence of a psychological purpose, is but one of many purposes that might give form and meaning to the incoming stimulations, and that the analysis of the "that" into sensations is the process that ensues normally when the more complex habitual interpretations prove inadequate. This is the normal method of "reconstituting the object". If the psychologist's sensations are artifacts, we may turn to consciousness in the concrete and find there the prototype of the psychological analysis into sensations which yields not artifacts but actual control factors that have meaning in a concrete situation. When we cannot depend on our "knowledge" that we have really locked the door, we go back and try it; if it does not yield, we go away believing it to be locked; but if doubt assail us once more we return, place the key in the lock, open the door, then close it and turn the key, concentrating attention to the utmost upon the kinaesthetic and visual sensations that are to form the basis of our assurance that we have really locked the door. The response of consciousness to stimulation as "sensations", occurs under certain definite, problematic conditions, when other reactions, whether conscious or unconscious, fail. When his interpretation of the "world" fails the philosopher, he may come back to the "facts of sense" from which to construct the world anew, but he gets from sense only that which comports with his purpose and his problem. The psychologist's "sensation" is but one of many potential "contents" that might be aroused by the same objective source of stimulation. And when the stimulation is reacted to as a "sensation" it is the exigencies of the moment that determine what "aspect" of the sensation is to be reacted to. There is nothing sacrosanct in the union of the "attributes". . . . We have noted further that certain historic conceptions that have come down to us, stand in need of re-definition. There may be a

wholesome efficacy in the scholastics' faith in the value of the unraveling of the implications of concepts. An historic analysis, coupled with an analysis of the concrete situations of human experience in which the current psychological concepts have their beginnings in the individual mind, is a method that may prove to make no mean contribution to our body of psychological knowledge. . . . A host of problems has been stirred up and there is much labor ahead; nevertheless, the time would seem to caution us to halt and re-examine the conceptions by means of which we would analyse and explain this somewhat, whether thing, or process, or function, that we call consciousness,—and then, having become clearly aware of the nature and the origin of these conceptions, return to the task anew.

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The Effect of Adaptation on the Temperature Difference Limen

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HISTORICAL SKETCH

Although the adaptation phenomenon has been recognized as an important factor in dealing with the temperature sense, very little systematic experimental work has been done in that direction. What has been done has been mainly in connection with work on the difference limen for temperature. The present survey does not attempt to cover the literature on the difference limen but only that which involves adaptation directly or indirectly, since the experiment reported was undertaken with a view to getting some light on the adaptation process itself and the difference limen was used as the most convenient indicator available.

Fechner,[1] working in 1855 on the application of the Weber law to temperature sensitivity, used two clay vessels of water in which the first and middle fingers of the right hand were immersed. He says, "The two fingers used for the experiment were first immersed in one of the two vessels until they reached a constant temperature, then immersed alternately in each vessel until a judgment had been made." Just what Fechner meant by a "constant temperature" it is difficult to guess. It may have been a state of complete adaptation or a state of relatively constant temperature sensation. He makes no allowance for this in computing his results but uses as his physiological zero the mean between freezing and blood temperature. Computing on this basis and using the method of minimal changes he finds the difference limen between 10° R. and 20° R. too small to give satisfactory conclusions but finds the Weber law may be applied betwen 20° R. and blood temperature.

Nothnagel[2] later investigated sensitivity to temperature differences by means of metal cylinders applied to the forearm and also by immersing one finger after the method of Fechner. At

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ordinary room temperature of 18° C. he finds that the temperature of the skin on the hand will lie usually between 33° C. and 36° C. Under these conditions the finest temperature difference perception is found with temperatures between 27° C. and 33° C. The difference limen increases slowly between 37° C. and 39° C. and very rapidly between 39° C. and 49° C. It also increases slowly between 27° C. and 14° C. and rapidly between 14° C. and 7° C. An ice bag was laid on the skin of one forearm for a period of from ½ to 1 hour and at the end of this time warm and cold were felt much less intensely on this area than on the other arm. Where there had been a difference limen of from .3° to .2° C. normally there was now a difference limen of from 1° to 3° C. The pain threshold for warm temperature was also raised from 49° C. to 54°-56° C. One hand was immersed for a period of from ½ to 1 hour in water of from 42° C. to 45° C. and the difference limen on this hand compared with that of the normal hand. The smallest difference limen for the normal hand lay between .2° and .1° while for the immersed hand it lay between .4° and .3° C. The pain threshold in this case was lowered from 47° C. to 46.5° C.

Thunberg[3] states that a finger may be adapted to 11° C. so that 12° C. will give a warm sensation and similarly a finger may be adapted to 39° C. so that a slight lowering of the temperature will produce a cold sensation.

Goldscheider,[4] in working with adaptation to temperature, was mainly interested in the effect of adaptation to extreme temperatures. He states that adaptation to extreme cold or warmth (15°-40° C.) blunts the temperature sensibility for both cold and warm. If one warms a cold spot the temperature cylinder will elicit a weak cold response for some time thereafter and one will regularly get a weaker cold response with the same stimulus if applied to a warmed area than if applied to a normal area. This holds also for warm points. He puts a finger in water of 15° C. and the corresponding finger of the opposite

[3] Thunberg, T., Upsala Läkaref. förh. 30, 1894-95, p. 521.

[4] Goldscheider, A., Physiologie der Hautsinnesnerven, vol. I, 1898. pp. 1-432

hand in water of 32° C. After 10 seconds both were plunged in water of 40° C. and the lukewarm finger felt the warmth more keenly than the cold finger. This is due, he thinks, to the fact that the extreme cold has blunted the sensitivity of the finger to warm as well as to cold stimuli. He thinks it probable that cooling lowers more the sensibility of the cold nerves than of the warm nerves and that warming lowers more the sensibility of the warm nerves than of the cold nerves. He adapted a finger to 40° C. and on plunging it into water of 30° C. he experienced a sensation of coolness but this was not as strong as the sensation of coolness which he experienced when he plunged a finger adapted to 30° C. into water of 20° C. In the same manner water of 30° C. will feel less cool to a finger adapted to 35° C. than 25° C. will feel to a finger adapted to 30° C. If a finger is adapted to 35° C. and then plunged into 30° C. it will feel less cool than a finger adapted to 36° C. and plunged into 30° C. This relation will hold for adapations to 37° C., 38° C., and 39° C. but with adaptation to 40° C. the sensation of coolness received from 30° C. decreases in intensity and with adaptation beyond 40° C. there will be no cool sensation received from 30° C. The similar boundary for cold adaptation is 21° C. Adaptation to extreme cold temperatures will also lower the sensitivity of the cold points to electric or mechanical stimuli. The effect of adaptation does not pass away immediately. Goldscheider found that if he adapted a cold spot to the anaesthesia point it would not react normally for an appreciable interval and that this interval was not lessened by exposing the area tested to a warmer medium during the period of recovery from adaptation.

Holm[5] experimented on the time duration of temperature sensations before complete adaptation. He applied a metal flask, with a thin bottom about 5 sq. cm. in area, to the skin and kept it there until complete adaptation was reached. The flask was filled with water kept at a constant temperature during the experiment and the following temperatures were used: 30° C., 25° C., 20° C., 15° C., 10° C., and 5° C., characterized as cold

[5] Holm, K. J., Die Dauer der Temperaturempfindungen bei constanter Reiztemperatur. Skand. Archiv. f. Physiol., vol. 14, 1903, pp. 242-248.

stimuli; 40° C. and 45° C., given as warm stimuli. Five tests were made with each temperature with the following results.

Average length of adaptation time for	was	Mean variation
45° C.	152″	8.2
40° C.	126″	20.8
30° C.	31″	3.2
25° C.	47″	3.6
20° C.	72″	2.4
15° C.	112″	12.4
10° C.	165″	6.
5° C.	210″	24.

It is to be noted that there is a marked increase in the mean variation as the temperatures rise over 30° C. and fall below 20° C. There is a correspondingly sudden increase in the length of the adaptation time for these temperatures. It is stated that during the time of adaptation the sensation fluctuates in that it disappears and reappears several times before it finally disappears permanently. Holm draws no conclusions from his results.

Alrutz,[6] in experimenting with conditions for perception of heat, found that under normal conditions a temperature of 41° C. applied to the arm gave a strong and unpleasant hot sensation. He adapted the arm for from three to five minutes to a temperature of 20.5° C. and when he again applied a temperature of 41° C. to the arm, a weaker, less unpleasant hot sensation was felt. Applied to an arm whose skin temperature was 32° C. a temperature of 14.5° C. gave a cold, unpleasant sensation. Applied to an arm which had been adapted three to five minutes to 41° C. a temperature of 14° C. gave a slower, weaker and not unpleasant cold sensation. In testing the threshold limen for hot he found that the limen differed with the initial temperature of the skin area to which the test was applied. As this temperature increased the hot limen rose and by artificially warming the skin the hot limen could be raised to 40° C. Similarly by adapting the skin to 22° C. the hot limen could be lowered to 32° C. The limen for paradoxical cold was also lowered by cooling the skin.

[6] Alrutz, S., Untersuchungen über die Temperatursinne, Zeit. f. Psych., vol. 47, pp. 241-286, 1908.

M. von Frey[7] calls attention to the fact that if an adapted area be subjected to a change in the intensifying direction a sensation will ensue. An "adapted temperature spot" will moreover respond to electrical and mechanical stimuli.

In 1910 Voigt[8] attempted, by experimental methods, to demonstrate the degree of objective adaptation which corresponds to adaptation which is subjectively complete. He argued that if adaptation were actually perfect he should be able to adapt one hand to 30° C. and the other to 35° C. until the two temperatures felt alike. He should then be able to plunge the 30° C. adapted hand into 35° C. and the 35° C. adapted hand into 40° C. and have them still feel alike. The fact that the two latter temperatures will not feel alike under such circumstances is evidence that adaptation is not objectively perfect.

He used in his experiment three enameled sheet iron vessels with double walls sixteen inches thick and with a capacity of three and three-fourths litres. The vessels were filled with water kept at a constant temperature by electric coils in the double walls or by ice in the water. The vessels 1, 2, and 3, stood next each other with 1 at the left of the observer. The left hand was submerged to the knuckles in 1 and the right hand in 2 for a period vary-from nine to fifteen minutes. At the end of this period the left hand was plunged into 2 and the right hand into 3 and an immediate judgment given as to whether 3 were higher, equal to, or lower than 2. Between 1 and 2 there was always a difference of five or ten degrees. The difference between 2 and 3 was varied, always in the direction in which 2 varied from 1, until a judgment of "no difference" was obtained. The amount of this difference was then compared with that between 1 and 2 and the ratio given as the degree of absolute adaptation attained.

Voigt, himself, was the only observer in the experiment and the temperatures were controlled by an assistant. The experiment covered about forty series of experiments, each series

[7] Frey, M. von, Physiologie der Sinnesorgane der menschlichen Haut. Ergeb, d. Physiol., vol. 9, 1910, pp. 351-369.

[8] Voigt, A., Über die Beurteilung von Temperaturen unter dem Einfluss der Adaptation. Zeit. f. Psych., vol. 56, 1910, pp. 344-355.

comprised of ten experiments. The following results were obtained.

Series No.	Initial adaptation temperatures 1 2	Final difference between 2 and 3	Ratio of difference between 2 and 3 to difference between 1 and 2
1	10°-15° C.	.5° C.	0.1
2	15°-20° C.	.5° C.	0.1
3	35°-40° C.	1.0° C.	0.2
4	40°-45° C.	1.0° C.	0.2
5	10°-20° C.	1.0° C.	0.1
6	30°-20° C.	1.5° C.	0.15
7	35°-25° C.	2.5° C.	0.25
8	35°-45° C.	1.5° C.	0.15
9	45°-35° C.	2.0° C.	0.2

Voigt also observes that adaptation to the point, where perception of difference between the adaptation temperatures vanished, never occurred even if a whole hour were given to adaptation. The maximum degree of adaptation usually took place within two minutes. He regards the degree of adaptation possible for the hands, as astonishingly slight and finds no proportional relation between different adaptation degrees and different points on the thermometric scale although he expresses the hope that with more elaborate apparatus and more extended investigation such a relationship might be found to obtain.

In taking the ratio of the actual difference between 2 and 3 to the difference between 1 and 2 as a measure of the degree of adaptation possibility Voigt is presupposing that the hand in 1 is as near perfect adaptation to temperature 1 as the hand in 2 is near perfect adaptation to temperature 2.

His results indicate that, with one hand adapted to 10° C. and the other to 15° C., .5° C. is near the difference limen for 15° C. If both hands were equally nearly adapted to their respective adaptation temperatures then such a method would give an indication of the relative potency of adaptation to different temperatures as judged by its effect on the difference limen. But there is no reason to suppose that adaptation is as nearly perfect for 10° C. as for 15°C. Beyond the fact that Voigt's method

demonstrates there is not perfect adaptation there can be no absolute determination of the degree of adaptation by means of the difference limen because any difference limen determined for temperature sensations must be determined under some condition of adaptation. This precludes any possibility of what might be termed a normal difference limen for purposes of comparison. Moreover, in series 1, 2, 3, 4, 5, and 8 Voigt is determining a difference limen in the direction of increasing thermometric temperature whereas in series 6, 7, and 9 the difference limen determined is in the direction of decreasing thermometric temperature. This fact makes these latter series incomparable with the others.

This completes the available experimental data on temperature adaptation. The present experiment undertook to investigate the comparative effect of adaptation to different temperatures on the difference limen for given temperatures, chosen more or less arbitrarily but extending from 40° C. to 17.5° C. at intervals of 2.5° C. Apparatus was devised for keeping water, the adaptation medium used, at any given temperature for an indefinite period.

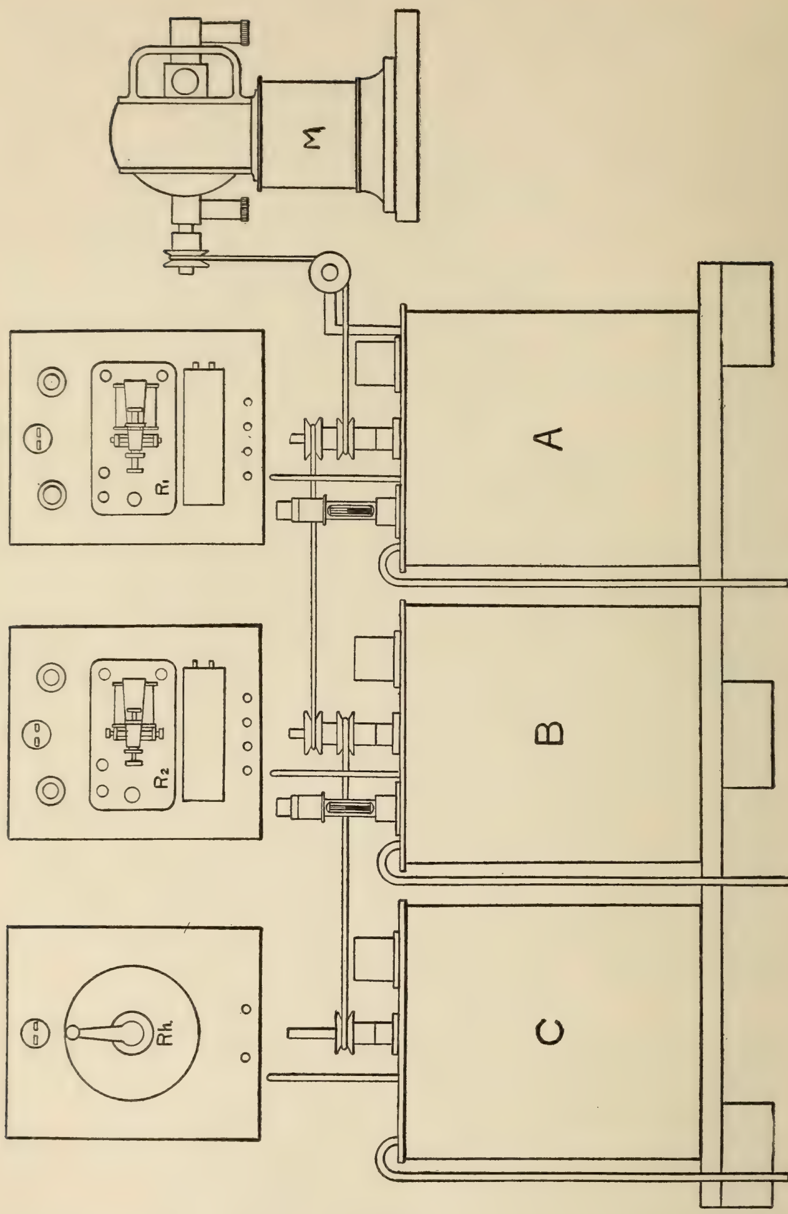


Fig. I.

A. B. C.—Stone Jars.
M—Motor.
R_1—Relay for Jar A.
R_2—Relay for Jar B.
Rh.—Rheostat for Jar C.

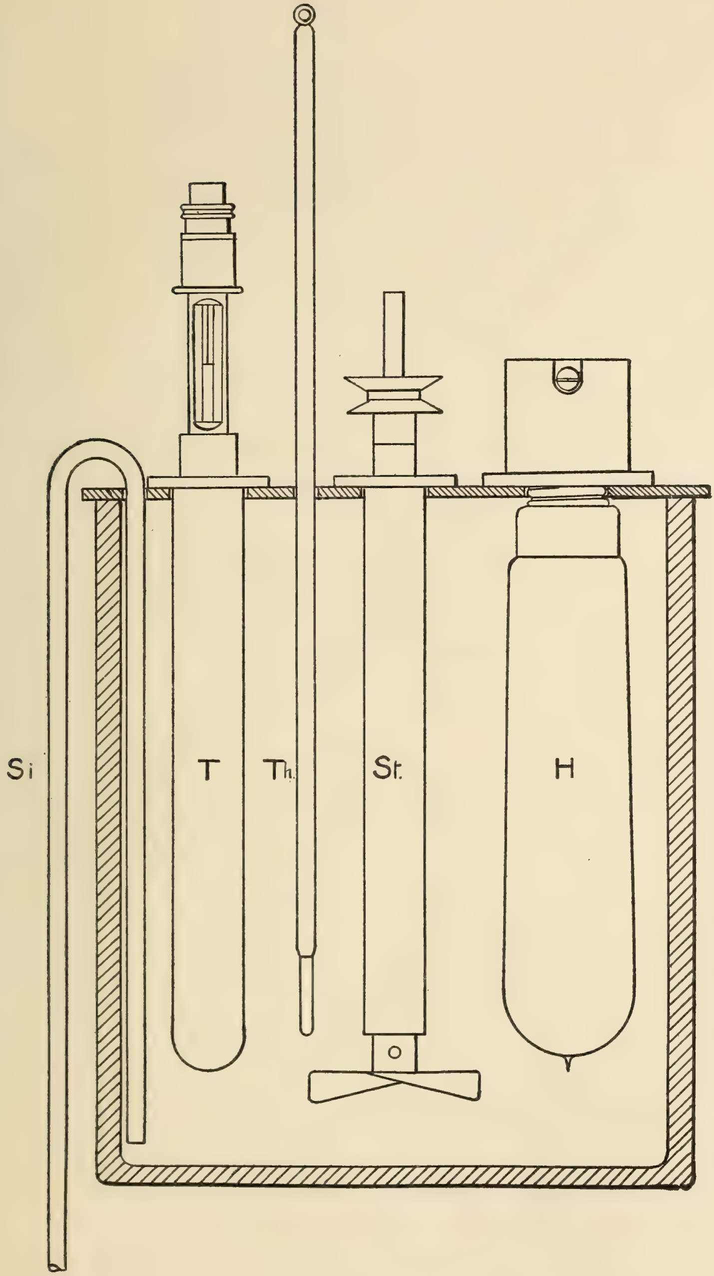


Fig. II.

H—Luminous Heater.
St.—Stirrer.
Si.—Siphon.
T—Thermostat.
Th.—Thermometer.

APPARATUS AND METHOD

The apparatus consisted of three round stone jars, A, B, and C, each holding four gallons of water. An asbestos cover was fitted to each jar and had, near one edge, an aperture, eight by one and a half inches long through which the hands were dipped into the water. Beside this aperture was a small hole through which a Centigrade thermometer, registering to tenths of a degree, was hung so that it registered the temperature of the water where the hands were dipped. From the centre of the cover depended a rod carrying a brass stirrer which was kept revolving by a system of pulleys connected with an electric motor. This gave the water motion enough to keep the temperature even but did not set up current enough to interfere with adaptation. The water was heated by means of a 500-watt luminous heater attached to the cover opposite the aperture for the hands. Near the heater was attached the thermostat. The thermostat consisted of a long glass tube extending down into the water the entire length of the jar and filled with mercury. The glass tube was attached to a steel cap having a hole through the center three-eighths inches in diameter. Inserted in this central hole and adjustable was a steel tube. Through the center of this steel tube was a small glass tube. The mercury on expanding was forced up into the glass tube and came in contact with a fine platinum wire which was adjustable. The mercury on making contact with the platinum wire completed an electric circuit which in turn by means of a relay closed the heating circuit. When the mercury dropped, breaking contact with the platinum wire, the heating circuit was again opened. The accompanying sketches [see Figures III and IV] show the details of this arrangement. The thermostat regulated to .1° C. for an indefinite period and was adjustable to any temperature.

The temperatures of A and B were controlled by thermostats. C was similar to A and B except that it had no thermostat and the heater was connected directly with a rheostat controlled by

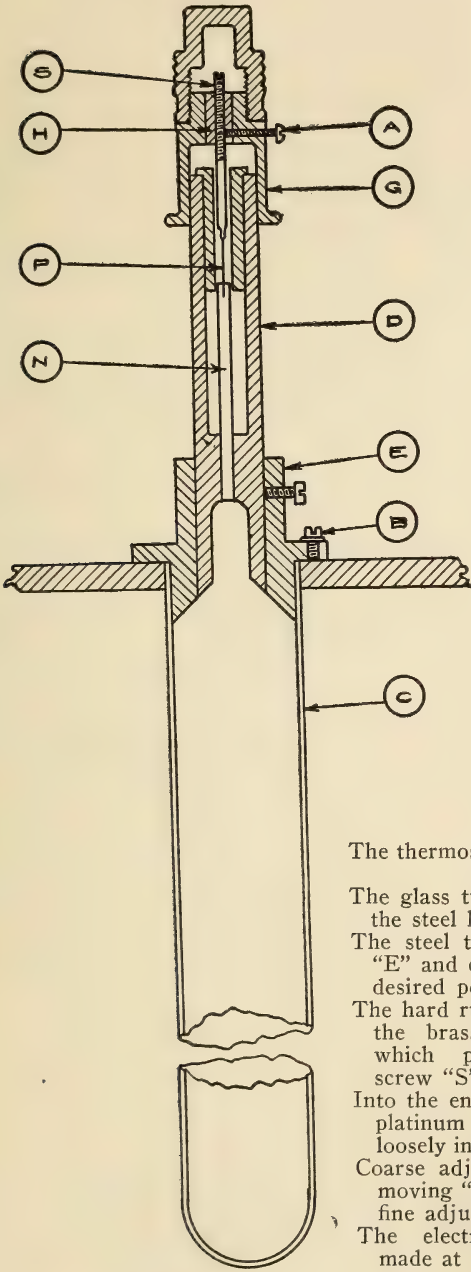


Fig. III.
The thermostat used in the experiment.
The glass tube "C" is attached to the steel head "E".
The steel tube "D" is fitted into "E" and can be adjusted to any desired position.
The hard rubber head "G" carries the brass plug "H" through which passes the adjusting screw "S".
Into the end of "S" is fastened a platinum needle "P" which fits loosely into glass tube "N".
Coarse adjustments are made by moving "D" up or down and the fine adjustments by screw "S".
The electrical connections are made at "A" and "B".

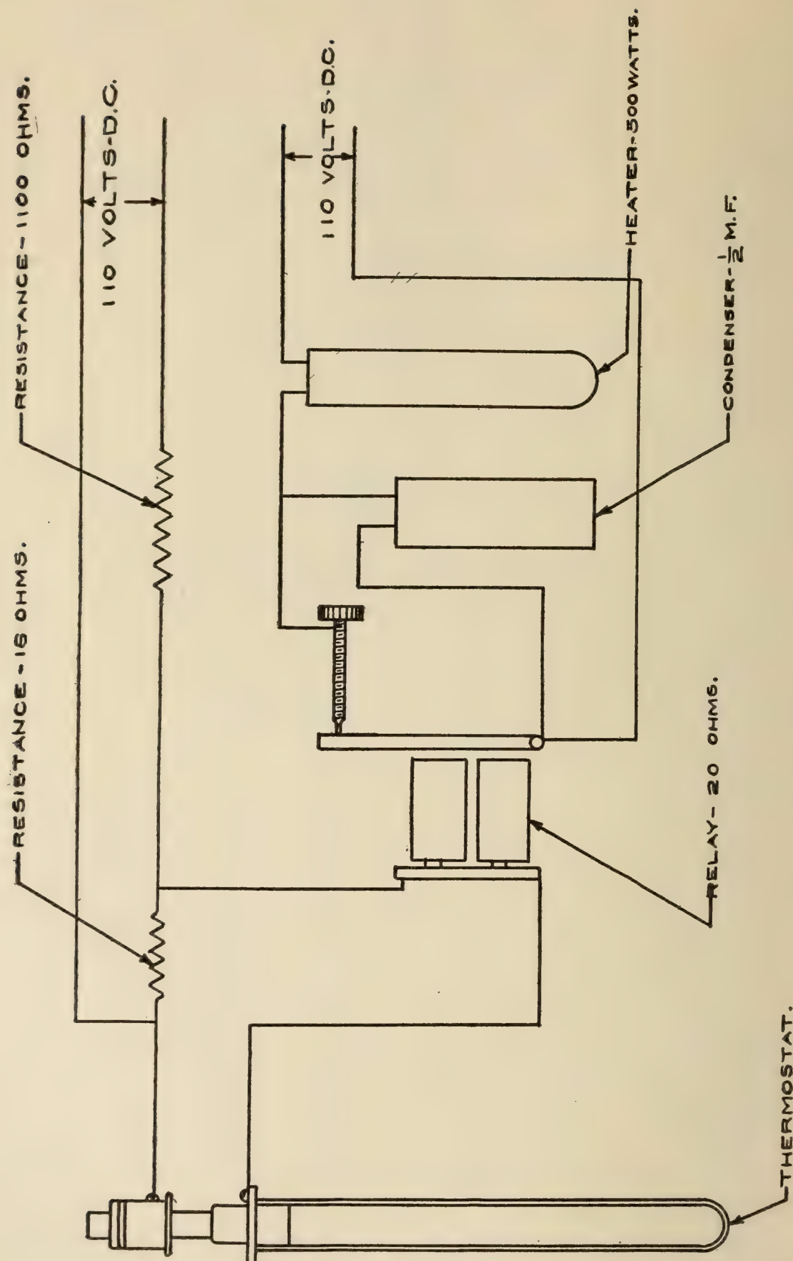


Fig. IV.
The electrical connections for the heater and thermostat.

hand. This was considered necessary at first because C was to be used mainly as the variable and it was not thought possible to vary temperatures and reproduce them with the requisite frequency by means of the thermostat. When we came to use the thermostats, we found that they were fully as satisfactory as the rheostat for this purpose. Jar A was always used as the adaptation jar and B and C were used in turn as the standard and variable in the determination of the limen. The subject was always in ignorance as to which jar was being used as the variable. The fact that the heating lamps were constantly shifting on and off made it possible to change the relative position of the standard and variable several times in the course of one experiment without the subject suspecting the change.

Water could be drawn off from the jars by means of siphons and when it was necessary to cool the temperature it was done by means of ice water poured in at the top. For the temperatures below room temperature ice was kept in the water and the thermostat set as usual. As the water kept cooling below the necessary temperature the thermostat kept it regulated.

The temperatures used throughout the experiment were 40° C., 37.5° C., 35° C., 32.5° C., 30° C., 27.5° C., 25° C., 22.5° C., 20° C., and 17.5° C. These limits were used to avoid the paradoxical cold in the upper temperatures and the painfully cold in the lower.

The method in general was the same for all the experiments. The observer stood before the adaptation jar A and rested his hands on the cover of the jar with the fingers dropped through the aperture so that the water reached to the second knuckles. The hands were held quietly until adaptation was complete when the subject raised his hands and dipped them, one into jar B and one into jar C. The subjects were cautioned against dipping their hands in above the second knuckles or dipping one hand in farther than the other or before the other. After a little practise they were able to follow directions. Every other judgment was taken with the hands crossed, the left hand in B and the right hand in C and every other judgment with the hands straight, the left hand in C, and the right hand in B. This was done in

order to avoid an association with the right or left hand. The hands were dipped in and taken out again immediately and the subject asked to give his immediate judgment. After the judgment was given the hands were returned to jar A to be readapted in preparation for the next test. The method of right and wrong cases was used in determining the limen.

Experiments were carried on with each subject three times a week for an hour at a time. At the beginning of each hour preliminary tests were made by dipping the two hands after adaptation into jar B to make sure that water of the same temperature felt alike to both. After three successive judgments of "alike" under such circumstances the experiments were begun. Only one adaptation temperature was used in the course of an hour. Four observers were used, all of them graduate students in the Department of Psychology of the University of Chicago and trained in psychological observation.

The length of time of adaptation varied considerably for different individuals and for the same individual at different times. Therefore there was no effort made to keep the time of adaptation constant. Adaptation was said to be complete when the observer was no longer conscious of sensations of temperature from the area submerged in water. Because the length of adaptation time depends on so many factors such as initial temperature, blood pressure and other possible physiological factors there was no attempt made to correlate it with the results. In general it may be noted that the first adaptation period during an hour's experimenting was noticeably longer than the subsequent periods.

In his experiments on adaptation Voigt notes that adaptation was never complete enough to make him oblivious of the difference between the two adaptation temperatures he was using. In some of our preliminary work, reported on at the annual meeting of the American Psychological Association in 1911, in which experiments were made involving simultaneous adaptation of the two hands to temperatures differing by 5° C. and ranging from 40° C. to 17° C., five different subjects, two of them trained observers, reported complete adaptation in six

hundred experiments. The four observers, used in the present experiment all agreed that a state of adaptation could be reached so complete that there was no sensation of temperature present. Although adaptation might seem more complete if both hands were in the same temperature than if they were in different temperatures, we have assumed on the basis of the previous experiments that adaptation may become subjectively complete and have used the introspection of the observer as a criterion of adaptation. Voigt was his own subject and states that he always knew the adaptation temperatures at the time of the experiment. It is possible that this fact accounts for his failure to obtain complete adaptation.

RESULTS

Introspective evidence on the process of adaptation

As has been said, the length of time of adaptation varied for individuals. Some subjects obtained complete adaptation within two minutes and others might have to spend twenty minutes in obtaining complete adaptation at the beginning of an hour's experimenting. In general the adaptation to extreme temperatures took longer than to moderate temperatures. The process of adaptation was marked, however, by certain characteristics which obtained for all subjects.

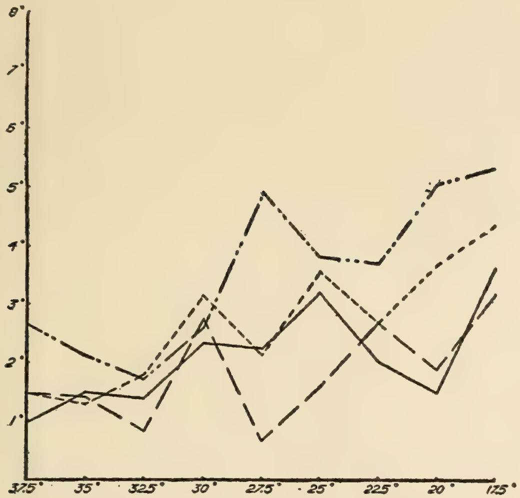
Adaptation did not take place gradually with a slow fading of the temperature sensation. There was scarcely any noticeable change in the sensation for a longer or shorter time, depending on the individual, and then the temperature sensation faded with remarkable quickness. It usually faded so quickly that the subject could give no adequate report of the process of its going. It merely was and then was not. But this first disappearance was not permanent. The sensation returned and there was likely to be a fluctuation in and out of consciousness for some little time before adaptation was complete and permanent. Each time the sensation returned it was a little fainter and lasted a shorter time. Holm[1] notes the same fluctuation in his account of the duration of temperature sensations before complete adaptation. The tips of the fingers were most sensitive and with 40° C. or 17.5° C. there was usually a tingling sensation in the tips of the fingers. This tingling fluctuated also but the fluctuations did not parallel those of the temperature sensation. Several of the subjects spoke of a feeling of heaviness in the fingers which increased as the hands remained in the water until just before adaptation set in, when the fingers felt noticeably lighter and more comfortable. With the disappearance of the temperature sensations the feeling of wetness disappeared and

[1] Holm, K. J., Die Dauer der Temperaturempfindungen bei constanter Reiztemperatur. Skand. Archiv. f. Physiol., vol. 14, 1903, pp. 242-248.

the fingers felt as though hanging in the air. The subject who experimented most with adaptation to extreme cold (17.5° C.) frequently noted the fact that the cold might sometimes be "referred to the water, that is it is the water that feels cold," and at other times the cold is "localized in the fingers. They feel as though they were the things that were cold." There was also with this temperature a dull ache as well as a sharp tingle and the two did not occur simultaneously but alternately.

The following report of an adaptation experience, taken at the beginning of a series with adaptation to 40° C., is typical of the adaptation process. "Pretty warm. Slightly tingling sensation. Another tingling starts at the bottom of fingers and goes up to wrist. Fingers feel heavy and soggy,—different from just wet. Tingling along outside of skin like an electric shock. Heavy fingers. Warm at upper end of fingers. Warmest just at edge of water. Tingling keeps on. Still pretty warm. Just about same temperature. Might be getting warmer. Creepy feeling goes up on skin especially on outside of little fingers. Still feels warm but perhaps not quite so warm. Warm at edge of water, not quite so warm. Tingling in left hand. Doesn't feel so warm any more—still warm just at edge of water. No temperature at tips of fingers. Little skin sensations of tickling, as though the skin contracted here and there—getting a little warmer. On the outside of hand near edge of water it is warm. Tingling sensation clear down at tips of fingers. About the same temperature, not quite so warm. More tingling especially on little finger. Heaviness has disappeared—feel lighter—pretty warm—not particularly warmer—slightly warmer near the edge of the water—now all feels cooler—warmth seems to settle on one finger and then on another—goes from place to place. No warmer—cooler if anything—not so much tingling—what there is is weak—does not go so far. Not quite so warm—tingle now and then—about the same, warm near the edge of water—no heaviness about fingers, general contracted feeling about the skin—pretty wet—warm current seems to come up against them but not so warm as has been—warm parts in middle and top—warmth when it comes is usually on back of

fingers—not so warm as it has been—once in a while a warm current comes along—very little warmth at all except at edge of water. When warmth comes now it does not seem so much at skin as on inside of finger. When fingers move it feels as though they were against sandpaper. Little warmer now. Still feels rather warm especially at edge of water. Not nearly as warm as at first. Rather warm, especially on back of little fingers. Heat is concentrated at back of fingers at edge of water—front part of fingers feels almost cool—slight tingle—fingers feel dried up—no warm currents—feel lighter, cooler—little fingers just a little warmer—still feels warm but not nearly so much as before—almost cool sometimes—still tingling cool in parts—twitches don't run up but localize in a joint—still warmth at edge of water—getting more moderate again—don't feel warmth much at all—just hazy feeling of warmth around edge of water. Warmth practically all gone except now and then on edge of little finger. Fingers feel as though it were warmth of blood rather than water. No more localized in fingers than in hand or in wrist—feels almost normal—adapted."

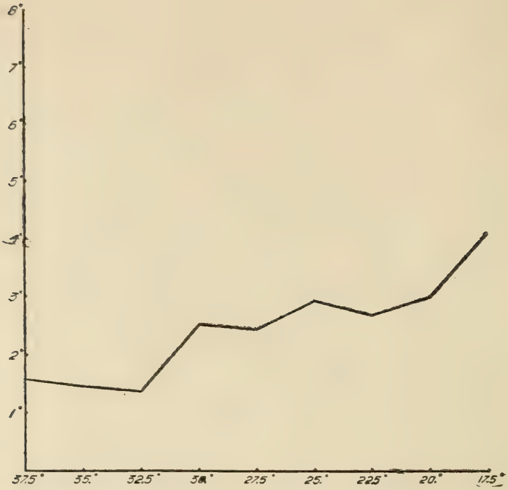


Graph I. Curve showing difference limen for different temperatures after adaptation to 40° C. The temperatures for which the difference limen was determined are represented on the abscissa and the amount of the difference limen on the ordinate.

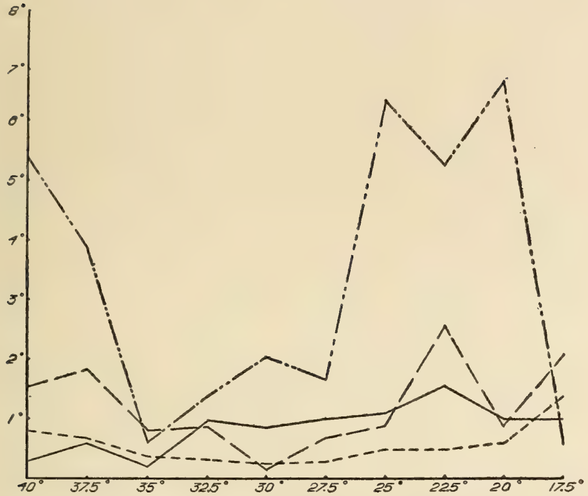
——— ——— ——— ——— Subject A
———————————— Subject B
—— — — —— — — —— Subject C
— — — — — — — — — — — Subject D

TABLE FOR GRAPH I

Temperature for which difference limen was determined.	Number of tests involved in determining difference limen.				Interval at which the percentage of right cases varied from the standard.			
	Subjects				Subjects			
	A	B	C	D	A	B	C	D
37.5° C	20	40	37	28	.1°C	.1°C	.1°C	.1°C
35	70	40	30	97	.1	.1	.1	.1
32.5	65	112	47	83	.1	.1	.1	.1
30	37	48	99	32	.1	.1	.1	.1
27.5	101	61	131	14	.1	.1	.2	.1
25	78	74	25	12	.1	.1	.1	.1
22.5	56	91	13	38	.1	.1	.1	.1
20	31	30	15	19	.1	.1	.1	.1
17.5	22	20	10	63	.1	.1	.1	.1



Graph II. Composite curve constructed from the individual curves of Graph I.



Graph III. Curve showing the difference limina for 40° C., 32.5° C., 27.5° C., and 25° C., under varying conditions of adaptation. The adaptation temperatures are represented on the abscissa and the amount of the difference limen on the ordinate.

Subject A ———————————————— difference limen for 40° C.

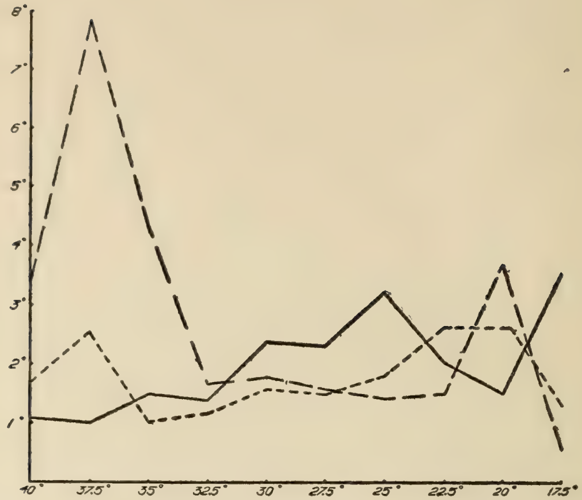
Subject A — — — — — — — — — — — — — — difference limen for 32.5° C.

Subject C ——— — — ——— — — ——— difference limen for 27.5° C.

Subject A ——— ——— ——— ——— difference limen for 25° C.

TABLE FOR GRAPH III

Adaptation temperature.	Number of tests involved in determining difference limen. Temperature for which difference limen was determined.				Interval at which the percentage of right cases varied from the standard. Temperature for which difference limen was determined.			
	40° C	32.5° C	27.5° C	25° C	40° C	32.5° C	27.5° C	25° C
40° C	16	65	131	78	.1° C	.1° C	.1° C	.1° C
37.5	23	54	44	36	.1	.1	.1	.1
35	16	38	22	52	.1	.1	.1	.1
32.5	38	26	28	38	.1	.1	.1	.1
30	26	41	34	71	.1	.1	.1	.1
27.5	12	40	42	25	.1	.1	.1	.1
25	25	41	69	33	.1	.1	.2	.1
22.5	24	43	50	62	.2	.1	.1	.1
20	25	53	71	28	.1	.1	.1	.1
17.5	33	29	21	28	.1	.1	.1	.1

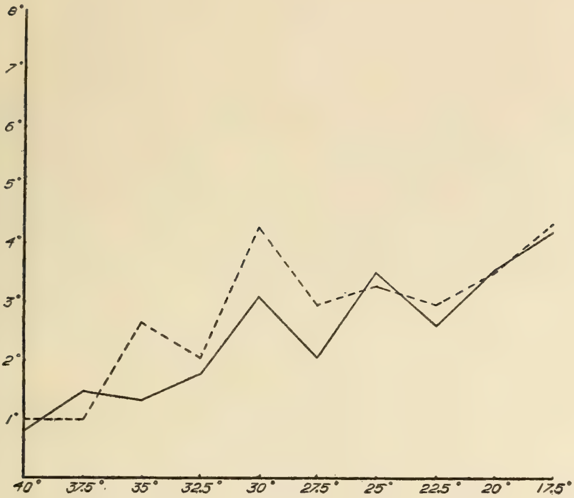


Graph IV. Curve showing difference limen for different temperatures after adaptation to 40° C., to 32.5° C., and to 17.5° C. The temperatures for which difference limina were determined are represented on the abscissa and the amount of the difference limen on the ordinate.

———————— Adaptation to 40° C.
— — — — — — — — — — — Adaptation to 32.5° C.
——— ——— ——— Adaptation to 17.5° C.

TABLE FOR GRAPH IV

Temperature for which difference limen was determined.	Number of tests involved in determining difference limen.			Interval at which the percentage of right cases varied from the standard.		
	Adaptation temperature			Adaptation temperature		
	40°C	32.5°C	17.5°C	40°C	32.5°C	17.5°C
40° C	39	38	57	.1°C	.1°C	.1°C
37.5	40	36	68	.1	.1	.1
35	40	30	52	.1	.1	.1
32.5	112	42	58	.1	.1	.1
30	48	46	16	.1	.1	.1
27.5	61	68	25	.1	.1	.1
25	74	45	33	.1	.1	.1
22.5	91	58	28	.1	.1	.1
20	30	53	46	.1	.1	.1
17.5	20	44	44	.1	.1	.1



Graph V. Curve showing the effect of adaptation to 40° C. on the difference limen compared with that of adaptation to 17.5° C. immediately followed by adaptation to 40° C. The temperatures for which the difference limen was determined are represented on the abscissa and the amount of the difference limen on the ordinate.
Subject D

———————————— Adaptation to 40° C.

— — — — — — — — — — — — Adaptation to 17.5° C. immediately followed by adaptation to 40° C.

TABLE FOR GRAPH V

Temperature for which difference limen was determined.	Number of tests involved in determining difference limen.		Interval at which the percentage of right cases varied from the standard.	
	Adaptation to 40° C	Adaptation to 17.5° C. followed by adaptation to 40° C	Adaptation to 40° C	Adaptation to 17.5° C. followed by adaptation to 40° C
40° C	38	19	.1	.1
37.5	20	62	.1	.1
35	97	49	.1	.1
32.5	83	40	.1	.1
30	32	38	.1	.1
27.5	14	51	.1	.1
25	12	56	.1	.1
22.5	38	41	.1	.1
20	19	15	.1	.1
17.5	63	27		

Objective Results

The first group of experiments, the results of which are represented in Graph I, was conducted with all four subjects. The adaptation temperature was constantly at 40° C. and the difference limina were determined for 37.5° C., 35° C., 32.5° C., 30° C., 27.5° C., 25° C., 22.5° C., 20° C., and 17.5° C. The ordinate in each case represents the difference limen in degrees Centigrade. Following the Graphs are tables showing the number of tests involved in determining each difference limen and the interval at which the percentage of right judgments exceeded or fell below the difference limen standard.

The main points to be noted in the curves are their similarity and the fact that there is a very consistent drop at 32.5° C. It should also be noted that although two of the curves drop between 22.5° C. and 20° C. there is a uniform and considerable rise between 20° C. and 17.5° C. There is a rise between 32.5° C. and 30° C. Between 30° C. and 20° C. irregularities occur both in the curves themselves and in their relation to each other although the general contour of the curves is remarkably similar even here. Graph II is the composite curve for the individual curves of Graph I. It brings out more clearly the points noted above.

Graph III represents the results of experiments carried on with two subpects. Subject C left during the college year and we were unable to finish the series begun with him. The curve of his results is comparable with the others of the Graph only in that it shows the same general relations. Because of individual differences in sensitivity no absolute comparisons can be made between it and the others. Each curve in this Graph represents the difference limen for a certain temperature under varying conditions of adaptation. The adaptation temperatures used were 40° C., 37.5° C., 35° C., 32.5° C., 30° C., 27.5° C., 25° C., 22.5° C., 20° C., and 17.5° C. The temperatures for which the limen was determined were 40° C., 32.5° C., 27.5° C., and 25° C.

The curve for the difference limen of 32.5° C. under varying conditions of adaptation is almost a straight line wth the ex-

ception of a sudden rise between 20° C. and 17.5° C. The curve for 32.5° C. also shows the smallest difference limen in every instance except with adaptations 40° C., 37.5° C., and 35° C., where the limen for 40° C., is slightly lower and with adaptation 30° C. where the limen for 25° C. is slightly lower. It should be noted that temperatures 25° C. and 40° C. are equidistant in opposite directions on the thermometric scale from 32.5° C. Under the same conditions of adaptation, however, their difference limina do not vary equally from that of 32.5° C. except with 32.5° C. and 20° C., adaptation where they are only .1° C. apart. Moreover the adaptation which gives the point of greatest sensitivity for 25° C. lies two and a half degrees below 32.5° C. and that which gives the point of greatest sensitivity for 40° C. lies two and a half degrees above 32.5° C. The two curves are not, however, uniformly converse. Several of these points are verified in other experiments with different subjects under slightly different conditions and will be pointed out again.

The facts which were so very apparent from this group of experiments, namely, that different conditions of adaptation had little or no effect on the difference limen for 32.5° C.; and that 32.5° C. seemed to be in general the region of greatest sensitivity to temperature differences while changes in either direction on the thermometric scale brought about a decrease of sensitivity with an increased susceptibility to variation under different conditions of adaptaton; these facts seemed to point to 32.5° C. as an adaptation which might furnish a norm for the difference limen for different temperatures which could profitably be compared with the difference limina for these same temperatures obtained under varying conditions of adaptation. Such a series of experiments was performed with subject B and the results are given in Graph IV.

This Graph verifies in an interesting way the observation made on the basis of Graph III that the condition of adaptation has little effect on the difference limen for 32.5° C. Three curves are given; one, the curve of the difference limina for 40° C., 37.5° C., 32.5° C., 30° C., 27.5° C., 25° C., 22.5° C., 20° C., and 17.5° C. with a constant adaptation temperature of 40° C.;

one, the curve of the difference limina for the same temperatures with a constant adaptation of 17.5° C.; and the third is the curve for the difference limina of the same temperatures with a constant adaptation of 32.5° C. The difference limina for 32.5° C. under these conditions lie within .5° C. of each other. The Graph, moreover, demonstrates that this is the only temperature of those used of which this is true since the limina for no other temperature lie so near each other.

It was noted in Graph III that with an adaptation temperature of 32.5° C. the difference limina for 40° C. and 25° C. were only .1° C. apart. In Graph IV the limina for the same temperatures under the some conditions, although greater than those for subject A, are again only .1° C. apart. This is a further verification of the results represented in Graph III.

One of the most interesting points of Graph IV is the converse character of the curves for adaptation to 40° C. and to 17.5° C. They cross close to 32.5° C., to 22.5° C. and again between 17.5° C. and 20° C. The curve for adaptation to 32.5° C. lies between the two except at 22.5° C. where it is above them both and between 27.5° and 35° C. where it lies below them both. In its general shape the curve follows the line of that for adaptation to 17.5° C. rather than for adaptation to 40° C. The same sudden change in the direction of the curves at 20° C. characterizes this as well as the other Graphs.

When we planned the experiment the results of which are shown in Graph V we had in mind the observation of Goldscheider (Physiologie der Hautsinnesnerven. 1898, vol. I, p. 143) to the effect that an adapted cold spot during the time of its recovery from adaptation was not affected by the medium in which it was placed during recovery. It seemed interesting to compare difference limina for 40° C., 37.5° C., 35° C., 32.5° C., 30° C., 27.5° C., 25° C., 22.5° C., 20° C., and 17.5° C. after an adaptation temperature of 40° C. with the difference limina for these same temperatures after an adaptation temperature of 17.5° C. immediately succeeded by adaptation to 40° C. If adaptation had a physiological basis involving different sets of endorgans, as Goldscheider's observation might lead one to

infer, then adaptation to 40° C. might conceivably take place before the effect of adaptation to 17.5° C. had disappeared and such an experiment would give us the effect of adaptation to both extremes to compare with that for the one extreme.

This experiment was carried on with subject D who reported that immediately on putting the hands into 40° C. after 17.5° C. scarcely any warmth was felt but that the warm sensation rapidly increased in intensity to a very warm and sometimes to a hot sensation and that the course of the adaptation process after this was as usual with 40° C. The time of adaptation to 40° C. did not seem to be affected.

The results of this experiment were rather unexpected. The two curves are practically coincident except between 35° C. and 27.5° C. Here the curve for the double adaptation is noticeably higher than that for single adaptation except at 32.5° C. where it drops close to the latter, again verifying the fact that conditions of adaptation have little effect on the difference limen for 32.5° C .

The results on the whole confirm the conclusion, deducible from the observations of other investigators in this field, that the difference limen for a given temperature is likely to vary with the variation of the preceding adaptation temperature. There are exceptions which will be discussed later

The only instance in which we have approximated a duplication of conditions is the work of Nothnagel[1], in which he obtained the difference limina on the hand for temperatures between 49° C. and 7° C. with the hand adapted at ordinary room temperature and with skin temperature between 33° C. and 36° C. This series is roughly comparable to our curve for difference limina between 40° C. and 17.5° C. with an adaptation temperature of 32.5° C. (Graph IV). He found the finest discrimination between 27° C. and 33° C. We find it occurring at 35° C. and slightly less fine discrimination occurring as low as 25° C. We also find a slow rise for temperatures below 27.5° C. as he did but he does not find the sudden drop that occurs in our curve at 17.5° C.

[1] Nothnagel, H., Beiträge zur Physiologie und Pathologie des Temperatursinns. Deutsch. Archiv. f. Klin. Med. II, 1867, pp. 284-299.

CONCLUSION

The theoretical discussions in the temperature field have centered in the past about the stimulus for the sensation of temperature. Briefly, the views advanced are: the Hering[1] view that the absolute temperature of the endorgan or skin is the determinant of the temperature sensation; and the Weber[2] view that change involving rising or sinking of the skin temperature is felt as warm or cold. The latter is sometimes restated in terms of the direction of the warm stream to or from the body. If the temperature of the skin were rising the direction of the warm stream would always be toward the body and away from it if the skin temperature were falling. Both of these views have been subject to modification but have persisted in essentially the same form throughout the literature. Both of them encountered the difficulty that we are sometimes not conscious of temperature sensation whereas in terms of the theories we should always be conscious of some sensation of temperature. If the sensation depended on the absolute temperature of the skin there is obviously never a moment when the skin is without absolute temperature. Similarly if the sensation depended on change of skin temperature the circulation of the blood and the exigencies of the environment cause a constant interchange of heat between the organism and the atmosphere or other objects as well as a constantly fluctuating skin temperature. No one who has had any experience with mechanical thermostats can believe that a living organism is ever in a condition of thermometric equilibrium for an appreciable interval.

Both views meet this difficulty with a concept which has come to be known as the "physiological zero." This is the name given to a physiological state which gives rise to no temperature sensations but variation from which will give rise to temperature sensations whose quality and intensity depend on the amount and

[1] Handb. der Physiol. III, 2, pp. 415-439 (1880).

[2] Wagner's Handwb. d. Physiol., V. III, 2, p. 481 (1846).

direction of the variation. From Hering's point of view the physiological zero is the result of an absolute skin temperature which has persisted long enough to cease to give temperature stimulation. A change of the absolute skin temperature to another above or below this point will result in a sensation of warm or cold. If this new temperature persists long enough the temperature sensation will cease and a new physiological zero will have been established. From his point of view adaptation is the establishing of such a physiological zero.

From the Weber point of view the physiological zero corresponds to any relatively stable skin temperature following upon a period of change. With the cessation of change the temperature sensation ceases, a physiological condition giving no temperature sensation is set up, and any succeeding change in either direction will give rise to a temperature sensation which will be cold or warm according to whether the change is down or up from the last physiological zero.

No one who surveys the accumulation of facts in the temperature field with either of these views in mind can fail to be struck with the fact that in either case there is an enormous proportion of contradictions and inconsistencies to the number of facts known. No sooner do we begin to state some concrete problem in the terms these views have made current than we find ourselves involved in ambiguous and self-contradictory statements. Part of this difficulty it seems may be avoided in the statement of the present problem by drawing a sharp distinction, as is done in stating problems in other sensory fields, between the objective conditions accompanying the sensation and the physiological conditions accompanying and determining the setting up of the impulse in the nerve leading from the endorgan.

The physicists tell us that heat is an interchange of energy between two bodies. It seems simple in that case to conceive of the temperature of the skin rising and falling as it receives or gives up energy to its environment and this change in temperature to be accompanied in consciousness by temperature sensations. But so simple a statement is entirely inadequate to describe the situation. The skin itself is made up of living cells each

one generating heat. The organism as a whole is generating heat which is carried to the skin by the blood. Conditions of environment differ constantly over the surface of the organism, both in regard to temperature and conductivity, vaso-motor conditions vary the blood supply constantly, and nutritive conditions also have their effect. The statement in mathematical terms of the heat potential of any given area of skin at any given moment would be a superhuman task involving all the factors already mentioned as well as that of relative mass of all the objects concerned, and even were it accomplished we should still be in the dark as to the physiological process by which the nervous structure of the endorgan is modified in the course of the exchange of heat or what relation this process bears to the thermometric scale. Were these different factors in the situation understood, the seemingly contradictory facts of temperature sensation could doubtless be explained in terms of them. But the time, when even a beginning at such an explanation can be made, is so far distant that it seems profitable to try to find other terms in which to describe temperature phenomena for the time being.

Suppose a skin area is placed in a medium maintaining a constant temperature of 30° C. There will be a period of temperature sensation which will shortly pass away and thereafter as long as the environmental temperature is kept constant there will be no temperature sensation. It is of course possible that physiological changes within the organism will so change the relation between environment and organism that temperature sensations will ensue but this factor can be minimized under experimental conditions and since it is a factor of which we can have no objective measurement it must be so eliminated. If the environment is now changed to 40° C. a temperature sensation will inevitably ensue. But if this new temperature is kept constant in its turn, the sensation will cease and no new sensation will occur until another change in the environment takes place. The situation may then be stated in this way. We have on the conscious side a period of no sensation followed by a sensation and again by another period of no sensation. Objectively we have a constant temperature of 30° C. followed by a change of ten degrees and

another constant temperature. The objective change in temperature is coexistent in time with the appearance of the sensation and with such a change a sensation will always normally appear. The temperature of the skin and in turn that of the endorgan may have changed ten degrees or five or next to none. We have no means of knowing exactly. Nor have we any means of knowing how the condition of the endorgan differs in the case of the second constant temperature from that of the first. We have no right to use such a difference as a term in describing the phenomenon. As the only available criterion of the intensity of the sensation we have the subjective evidence of the observer. All that we can say with definiteness is that change in temperature of the environment is accompanied by sensations of temperature and that under a constant temperature condition of the environment sensations of temperature cease. Adaptation to temperature, stated in these terms, will mean absence of temperature sensations and a constant environmental temperature. The amount of change and duration of constancy of the environmental temperature happen to be things we can control experimentally. Therefore they are comparatively safe terms to use for the present.

The point which the results of the present experiment bring out most clearly is the fact that the explanation of adaptation by the conception of a physiological zero in the form of an actual physiological state shifting up and down with external conditions much as the mercury shifts up and down in the thermometer and determining the quality and intensity of the ensuing sensation, is not compatible with the facts. If such a physiological phenomenon did occur we would expect it to have an effect on the difference limina determined with different adaptation temperatures. That is, if we found a difference limen of 1° C. at 40° C. with an adaptation of 30° C. we should expect to find a smaller limen at 40° C. with an adaptation of 35° C. for we have brought the physiological zero 5° C. nearer to 40° C. and, in the old terms, 40° C. would give less variation from the physiological zero, the sensation would be less intense and difference more easily perceived. And a glance at the graphs

will show that we find something quite like this. But if this change in difference limen were the effect of an actual physiological condition, we should find some evidence of shift in the difference limina all through the thermometric scale for we have shifted the physiological zero with relation to all temperatures. Instead we find a certain definite point on the thermometric scale where conditions of adaptation have practically no effect whatever. This is 32.5° C., the point of greatest sensitivity to temperature difference. The resistance of sensitivity at this point to any and all conditions is brought out in all the curves. This makes one dubious of the shifting physiological zero. The curves do seem to show, however, that there is a point on the thermometric scale which may be taken as a norm where shift in external temperature is most readily recognized and from which sensitivity to change in temperature decreases in either direction. This temperature (32.5° C.) is very close to 32° C., which is given by Benedict and Slack[1] as normal skin temperature. It is to be regretted that 32° C. was not one of the temperatures used but the temperatures were chosen rather arbitrarily at the beginning of the experiment, there being nothing particularly to guide a choice. We shall refer to this temperature as skin temperature since the term "physiological zero" has acquired such a distinctive connotation.

From a biological point of view the persistence of skin temperature, as a more or less constant basis for appreciating variation of temperature, is a much more possible conception than the older one of a shifting physiological zero. If an organism, in whose life processes the temperature factor were vital, should move into a temperature warmer than normal, and the physiological zero should then move up so that an increase of warmth were only slightly sensed, and this in turn should produce a higher physiological zero, there would be nothing to prevent the organism moving on to its destruction. Such a psysiological zero in any event would never be useful and might be distinctly harmful.

[3] Benedict, F. G., and Slack, E. F., A Comparative Study of Temperature Fluctuations in Different Parts of the Body. From the Nutrition Laboratory of the Carnegie Institution of Washington. 1911.

The problem remains of explaining the varying effects of different adaptation conditions on temperatures above and below skin temperature. Suppose a subject is in a state of adaptation to skin temperature and the objective temperature is raised to 35° C, The subject appreciates it as warmer than skin temperature, but this objective temperature again remains constant and the temperature sensation disappears. Shortly the objective temperature is raised again to 40° C. This again is judged warmer than skin temperature but this time the judgment is made in terms of the last increase from skin temperature, that is, it is a genuine perception. It is very evident that any such judgment would be modified by the last previous sensation and would vary as it varied. Such processes, involving the use of past experience, where the whole mechanism of comparison and judgment has dropped from consciousness leaving only the final result registered in consciousness as a unit, are frequent in all the other senses and there is no reason to suppose that such a process should not have been built up in the temperature sense which is one of the oldest. That it has never overtly appeared in normal human consciousness is no more an argument against it than it is against the perceptual character of the processes involving visual space. It is of great biological importance that such a process should be built up for, as an organism passes through varying temperatures, it will have to know not only that each succeeding temperature varies from its normal habitat but whether it varies more or less than the last one it experienced. Else how would it know it was getting nearer to or farther from where it ought to be?

We have somewhat complicated the situation in our experiment by making it necessary for the subject to judge between two temperatures both varying from skin temperature and to do this in terms of a third (the adaptation temperature), which gave rise to the sensation immediately preceding the adaptation experience and which also varied from skin temperature in most cases. Occasionally we presented him with two temperatures one of which was skin temperature itself. The results (see Graph III) show that under these circumstances the judgment not only im-

proved but that for all such experiments the judgment was practically the same.

We should expect that, if this is a true description of the situation, we would find that adaptation to 40° C., for instance, would give a rather better discriminative judgment as the discriminated temperatures approached 32.5° C. from 40° C. since they would be judged as varying from skin temperature less than 40° C. while beyond the skin temperature in the other direction the 40° C. adaptation would only prove confusing. A glance at the curves will show that this is in general the case. The curve on Graph IV, for instance, which shows the difference limina for different temperatures with an adaptation temperature of 17.5° C., is noticeably low and uniform between 22.5° C. and 32.5° C. where it begins to rise rapidly. We should also expect to find that if an adaptation temperature of 40° C. were used the temperatures between 40° C. and skin temperature would seem somewhat less intensive and the discrimination judgment therefore finer than if an adaptation temperature of 32.5° C. were used. The same thing should hold on the other side of skin temperature if an adaptation temperature of 17.5° C. were compared with that of 32.5° C. The curves of Graph IV substantiate this in the main although rather more clearly for 40° C. adaptation than for 17.5° C. adaptation. Another effect should be that the influence of an adaptation temperature on the discrimination between temperatures on the opposite side of the skin temperature would vary rather more in the case of temperatures near the skin temperature than of those at a distance since it would probably have very little effect on those at a distance, while it would effect more or less those near by. This too proves to be the case as the curves indicate. The last curve (Graph V) is particularly striking in this respect for the extreme cold adaptation temperature preceding the extreme warm adaptation temperature has proved confusing only for the temperatures near skin temperature.

The factor, which influences the sensation ensuing on a period of adaptation to temperature, seems to be, not the adaptation experience itself, but the sensation immediately preceding the adaptation experience.

It may be offered as an objection to the present statement of the adaptation situation that one may get a cool sensation from 35° C. for instance, after adaptation to 38° C. This may be explained as an illusion of the kind which is always found in connection with this type of perceptual process. The temperature 35° C. is judged as varying from skin temperature but so much less so than 38° C. that it is shunted over the line and judged as varying in the opposite direction. The fact that this phenomenon is possible only within certain limits on either side of skin temperature and that these limits may be somewhat extended by introducing the contrast effect as in the experiment in which one hand is plunged into cold water, the other into warm and then both into lukewarm water, is a further indication that it belongs to the illusion type of process.

Since the experiments of Goldsheider and von Frey on the localization of warm and cold spots, it has been generally held that there are separate systems of endorgans for warm and cold sensations. The recent experiments of Head, resulting in a division of cutaneous sensations into epicritic and protopathic have suggested the possibility of four systems of endorgans for temperature sensations. The difficulties of making the physiological zero concept fit such a multiplicity of endorgan systems are obvious. The concept, itself, conceived primarily to explain the adaptation phenomenon, has failed signally to make itself compatible with the facts involved in adaptation under the present experimental conditions. When these facts, moreover, are stated in terms of objective environmental conditions on the one hand and of conscious processes on the other hand any such explanation as the physiological zero becomes needless. That is to say, there seems to be no more reason to suppose that a physiological condition accompanying a state of no-temperature-sensation should affect the quality and intensity of the ensuing temperature sensation than to suppose that a physiological condition accompanying a state of no-pressure-sensation should affect the quality and intensity of the ensuing pressure sensation. Such a supposition would differentiate the temperature sensation from all other sensory fields for in other sensory fields this is true only in the case of a

fatigued endorgan and we have already quoted von Frey to the effect that the state of no-temperature-sensation cannot be due to fatigue. Since we can find no parallel for the physiological zero concept in other sensory fields, since it does not fit the facts in the temperature field, and since the facts it attempted to explain may be explained without it, the concept seems untenable.

When the experiment was first undertaken it was thought possible that the results might shed light on the applicability of the Weber Fechner law in the temperature field. If the proferred interpretation of the results is correct, under the conditions of the experiment the only curve which could bear on the question is that for adaptation temperature 32.5° C. in Graph IV. This curve is not in acordance with the Weber Fechner law but it is only one curve for only one subject and no conclusions can be drawn on such slight evidence.

The experiments were not of a nature to throw any light on the duality of endorgans for the temperature series, although the fact that below 20° C. the character of the discriminative judgment often undergoes a marked change, is suggestive. In this connection we may draw attention to the fact that Holm's[4] results for duration of temperature sensations before complete adaptation, show a sudden increase in mean variation and length of adaptation time as the adaptation temperature falls below 20° C.

We may add, in closing, that this is not an attempt to burden the world with a new temperature theory. It is an effort to state the problem in terms which may be used to describe two or more facts without automatically setting the facts by the ears.

[4] Holm, K. J., Die Dauer der Temperaturempfindungen bei constanter Reiztemperatur. Skand. Archiv. f. Physiol., vol. 14, 1903, pp. 242-248.

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THE TONOSCOPE

BY

CARL E. SEASHORE

In experimental psychology instruments have as a rule been designed to meet immediate needs, and have usually been described incidentally in reporting the results of psychological investigations. Much research has been wasted because done with untried apparatus. In fact most of our instruments are in a crude condition; and many fields of investigation lie untouched for want of measuring instruments. It is a sign of a higher stage in the science that the most essential psychological instruments are now being subjected to investigation apart from the specific pending psychological use. Only in this way can we properly develop instruments and standardize the technique of manipulation. I therefore take pleasure in presenting the description of an instrument and its use, as an object worth while in itself.

The tonoscope, Fig. 1, works on the principle of stroboscopic vision, the principle of moving pictures. Auditory vibrations of air, caused by voice or musical instrument, are converted directly and instantaneously into visual configurations on a screen, and the vibration frequency which denotes the pitch of the tone may be seen in plain figures on a scale. This enables us to measure the pitch of any tone by direct inspection while singing, speaking, or playing under normal conditions. The ability to do this opens up countless problems in the psychology of tonal expression.

There is a contrivance by which the vibration of the voice mechanically raises and lowers a flame for each sound wave. The oscillation of the flame results in corresponding exposures on the screen which it illuminates. The vibration being rapid, the retinal lag produces the effect of continuous vision, although the duration of the illumination for each vibration is very short in comparison with the corresponding period of non-illumination. In moving pictures it is well known that, if we have successive pictures which are alike thrown on the screen in the same place and in rapid succession they form one continuous picture which stands out clear and still. This is the principle here employed. The revolving screen,

rotating at the rate of one revolution per second, carries rows of dots, regularly spaced but varying in number for each row. When a tone is sounded, the row which has the dot-frequency that corresponds to the vibration-frequency of the tone will stand still and be clear while all other dots move and tend to blur. Each row runs under a number on the scale. The row which stands still, therefore, points to a number which designates the pitch of the tone. The screen contains a sufficient number of rows of dots, varying in number, to correspond directly, or by multiple, to all tones within the range of the voice. To see the pitch of the tone one has therefore only to see the number of the line that stands still.

Earlier models of the tonoscope have been described in a previous volume of these Studies (1); also in the Musician (2). Such radical changes have, however, been made since then that we are now dealing with an instrument very much modified and extended in its usefulness.[1] The present instrument is not the result of the work of one man but of many of whom, aside from those who have developed the principle of stroboscopic vision in physics, I desire to mention particularly Dr. E. W. Scripture who designed the first laboratory exercise using this principle in psychology (3); Dr. C. F. Lorenz (4), to whose ingenuity and most generous coöperation we owe the synchronous motor and the plan of using the selenium cell with the siren; Mr. E. W. Bechly, and Professor E. A. Jenner, who made the first tests with the tonoscope in determining its value for use in the musical conservatory (5); and Dr. Walter R. Miles (6), who has standardized procedure for various problems in the measurement of singers with this instrument.

Instead of giving merely a description of the commercial form of the instrument, I shall attempt to suggest, in a semi-technical way, its various possible forms on the basis of actual experiment from the laboratory point of view. The essential features which must be discussed in turn in the description are the speed regulation, the screen, the dot grouping, the sensitive light and sound transmitter, and the siren.

The synchronous motor.—The validity of stroboscopic frequency measurements depends upon the accuracy with which the movement

[1] Fig. 2, showing the 1902 model, is here reproduced because it shows the bare elements of construction better than they can be seen in the present encased model, as shown in Fig. 1.

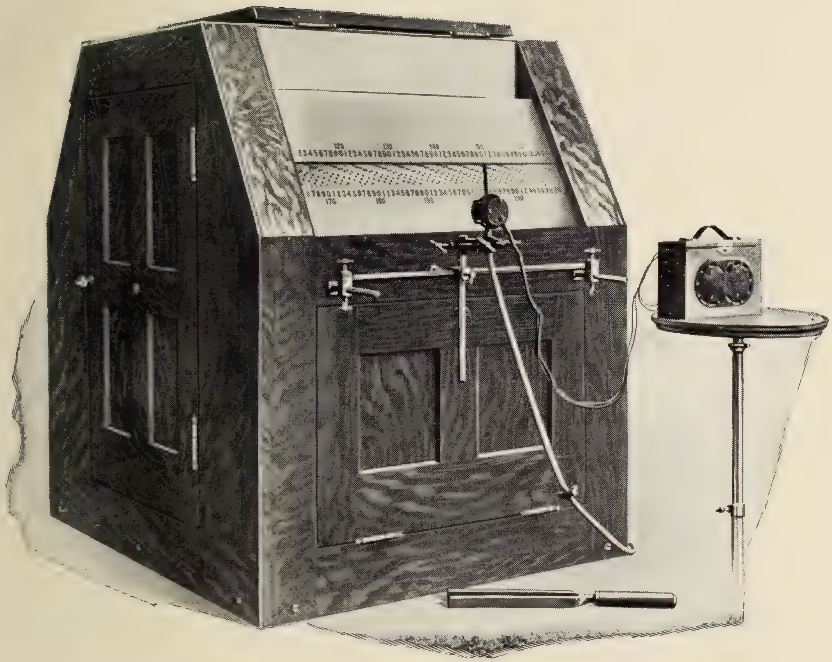

FIG. I. THE TONOSCOPE

(The instrument seen at the right is the acousticon.)

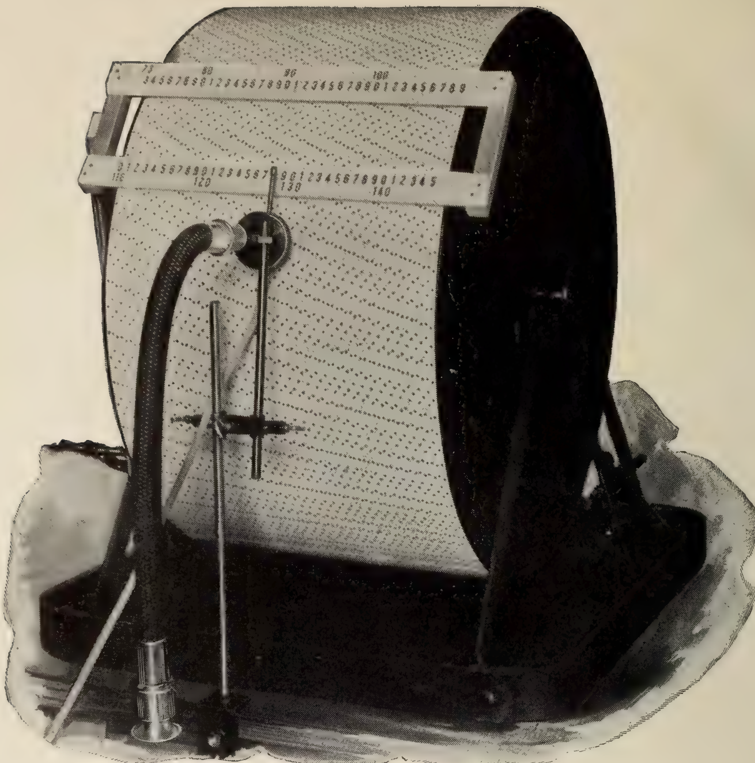

Fig. 2. Early Model of Tonoscope. (1902)

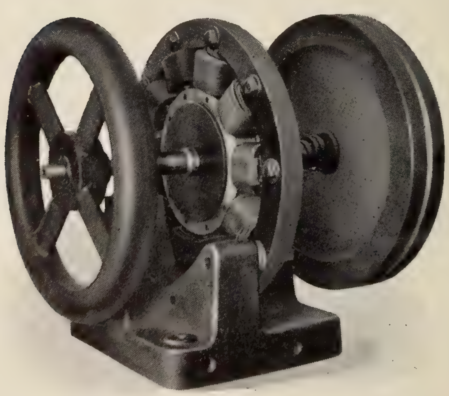

Fig. 3. The Synchronous Motor

of the exposed object is controlled. The method employed in the early models proved entirely too laborious and inconvenient.[2] The final solution was found in the use of a synchronous motor which drives the drum (screen) at a regulated speed. This motor (Fig. 3) works on the principle that, if a regularly interrupted current be sent through a multipolar field, and the needed initial momentum be given to the revolving multipolar armature, each closing of the circuit will synchronize with poles always in a corresponding position of approach, and the momentary pull will be sufficient to continue the rotation until the next pull occurs, at the next approach of poles. This is the principle of La Cour's "phonic wheel" (7), a principle also employed by Lord Rayleigh (8).

A motor of this type is mounted on the main shaft of the tonoscope drum.[3] The drum, serving as a balance wheel and being connected to the motor by a coil spring, furnishes the right degree of inertia and flexibility in the transfer of the pull.[4]

A large 10 v.d. tuning fork is used as an interrupter. It is energized by primary cells, and is encased in a box which is kept out of the way in a closet so that no noise shall come from it. A 16 c.p. lamp used for resistance in the motor circuit, is mounted

[2] In the early models the drum was driven by an ordinary direct current motor. An assistant at the back side of the drum observed and recorded the actual speed at the time of every reading. This was done by means of the stroboscopic effect of the intermittent light in a vacuum tube in circuit with a standard tuning fork. Every reading had to be adjusted with reference to an elaborate table of corrections calculated for different steps of variation from the true speed.

[3] In the early stages the motor was placed outside of the drum and the power was transferred by belt. This was neither as convenient nor as reliable as the present method, but it left the motor available for other laboratory purposes.

There is a great demand for such a synchronous motor in laboratories because, with constant speed and power, many of the laboratory problems are readily solved and we gain a higher degree of accuracy than can be obtained by any other form of electric motor or by kymograph clockworks. As the speed may be transformed up or down, this motor will take the place of many cruder devices in the physics and psychology laboratories for securing constant motion, marking intervals, measuring time, making exposures, etc.

[4] In the type of motor here needed this principle has not before been successfully developed for practical use. The hitch lies in the jerky nature of the pull. By attaching a flywheel to the axle by a flexible connector we get a flexible moment of inertia which solves the problem.

between the prongs of the fork and proves a convenient means of keeping it at a sufficiently constant temperature, the temperature being that to which the fork is raised by the heat of the lamp within the box.[5]

A 110 volt direct current is completed through the motor and a mercury contact interrupted by the fork. The current is reduced by the lamp resistance. The make-and-break is short-circuited with a condenser to avoid forming of an arc. A large amplitude of the fork, fully 10 mm., also helps in preventing the tendency to arc.

A rheostat inside the tonoscope case, with a switch on the surface, serves for the adjustment of current, as there may be fluctuations in the supply main. A small detachable crank for starting fits the end of the main shaft which comes out flush with the edge of the case on the side. To start the tonoscope one has only to start the fork, give the drum a turn up to approximately one revolution per second and close the switch. Once started, the instrument will run indefinitely and there is no care or distraction in the running of it.

The screen.—The stroboscopic screen is formed by mounting a sheet of aluminum in the shape of a drum over a heavy balance wheel. A section of this drum is seen through the opening on the front of the case (Fig. 1). This screen is 50 cm. wide and has a circumference of 242 cm. The balance wheel is heavily mounted on ball-bearings resting on a heavy iron frame. The whole instrument is enclosed in an oak case with doors on every facet.

The size of the drum is determined by the minimum area for the legible distribution of 18,500 markings, or stroboscopic dots. In the present screen the dots are bored holes, three and one-half mm. in diameter. The inside of the drum being dark, the holes show up clearly as black spots on the light aluminum surface. These holes are spaced with the highest mechanical accuracy and are arranged in 110 parallel rows, each completing the circumference of the drum in uniform spacings for each row (Cf. arrangement of dots in screen in Fig. 2). One row has 110 dots and the dot frequency in the remaining rows increases by one dot for each row up to and including 219. Thus we get frequencies to correspond

[5] Like the motor, this fork becomes a sort of "universal" apparatus in the laboratory. Being standardized and always connected up, it becomes the most convenient means for timing purposes. It is notably serviceable in connection with a multiple recorder which makes impressions on ticker tape.

to each intergral vibration-frequency in an octave of tones, the octave of 110 v.d. to 220 v.d. This is approximately the octave from *A* up to the *a* below middle c̈

This octave was chosen after much experimenting as being the most serviceable, all factors taken into consideration. Within this octave the tones are read directly, and above and below it they are read by multiples. The number of holes in each row is shown in plain large figures on the scale. When the drum revolves the row formation stands out clearly and each row points to a number.

The "framing-effect".—As may be seen in Fig. 1, there is an upper and lower scale, one on each edge of the shield. It is necessary that the holes should be large enough to be easily legible under the prevailing conditions of fusion, and also that they shall be widely enough spaced in both directions to be easily read. It is also essential that a single little sensitive flame shall light up the whole exposed surface of the screen. This forces upon us the difficult problem of securing compactness. In the early models no other solution was seen than to restrict the instrument to a part of an octave, but a unique solution was finally found. This consists in alternating rows of widely different frequency as may be seen by observing the actual numbers of the adjacent lines on the screen in reading alternately on the upper and lower scale. The numbers on the upper scale are consecutive from 110 to 164, and the numbers on the lower scale from 165 to 219, the rows reading on one scale alternating with those reading on the other. To illustrate, if rows 150 and 151 were adjacent they would need to be separated by a wide space in order to be differentiated, because their movements are so nearly alike, but if they are separated by another row, for example the 205, the differentiation between the two original rows becomes clearer and the spaces between them may be materially reduced, for, when rows 150 and 151 stand approximately still and their individual dots stand out clear and distinct, row 205 moves so swiftly that it forms one continuous line or gray streak which has a most serviceable separating or framing effect for the adjacent rows. When the rows are arranged as shown on the scale, this differentiating, or framing effect will operate for each and every row that may be standing out for reading. This contrivance makes it possible to reduce the screen to about one-third of the size otherwise required, and still makes the reading more legible than it would

have been on a screen three times as large without some contrivance like this.

The sensitive light and sound transmitter.—A fundamental requirement in this principle of measurement is that the light shall be made intermittent through the action of sound waves. This may be accomplished in various ways. In the simplest arrangement an ordinary manometric capsule is used and the singer holds a funnel before his mouth in such a way as to effectively collect the vibrations. Acetylene gas supplied by a motorcycle tank is used for this sensitive flame. We have not yet determined the most effective form of capsule or the maximum upper and lower limit of its vibration response, but have found that this varies with numerous conditions, such as the vibration frequency, the volume, the smoothness, etc. of the tone. This capsule may be used in recording from such musical instruments as send a fairly concentrated volume of waves in one direction, such as tuning forks, wind instruments, reed instruments, and the siren. With all these it is, however, advantageous to use a Helmholtz or a Koenig resonator as a selector although it is not necessary in all cases. As a rule the shorter the speaking tube or horn, the less danger there is of interference in the sound waves.

While this mechanical transmission through a manometric capsule is for most purposes the simplest means, and is entirely satisfactory, especially in singing, we have electrical devices that have distinct advantages. The receiver of a microphone may be converted into a manometric capsule by building a gas chamber on the ear side and supplying it with a gas inlet and a jet nipple. The vibration of the receiver membrane controls the gas flame in the same way as in an ordinary capsule. The microphone transmitter is used with this as in ordinary speaking. The best type of commercial instrument that may be readily adapted for this purpose is the phonette or the acousticon made by the General Acoustic Company, New York. The acousticon known as type D seems to be the most serviceable.

While the electrical apparatus may be a little more delicate to handle it has the advantage that it is more sensitive and can be used for the recording of a tone which would not be strong enough to register in any other way. It also makes it possible to set this apparatus in front of the singer so that he may sing for a record

without being aware that a recording instrument is present in the room. The singer may be isolated in a quiet room or in familiar surroundings, in order not to be disturbed by the presence of another person and the main instrument. The measurement may even be made at any long distance covered by telephone connection, as all that is necessary is to put the microphone transmitter in front of the singer at one end of the telephone line, and connect it with the microphone receiver on the tonoscope at the other end.

When sound vibrations are strong enough completely to make and break the circuit, the ordinary telephone receiver may be used as a capsule in the manner just described. On certain musical instruments a mechanical interrupter resting on the resonating chamber of the instrument, for example, a violin may be used.

Under the same circumstances a vacuum tube may be used in place of the gas-flame capsule. The intermittent light is then caused by the interruption of the current in the primary circuit of an induction coil which has the vacuum tube in the secondary circuit.

If two simultaneous records are desired, one record may be taken on each side of the tonoscope. Indeed, four records may be taken simultaneously by using both the upper and the lower facets on the back and the face of the drum, there being doors on the case for this purpose.

The stroboscopic reading requires fairly complete darkness. To avoid darkening the room a hood (not shown in the figure) has been built to fit over the reading surface of the tonoscope. This hood forms a dark chamber and the inner surface, being bright, serves as an excellent reflector for the light. For intensive reading at a given point on the scale, a small sliding hood is made on the same principle. It has the advantage of centering the light upon the point of reading in the scale. A reflecting mirror (not shown in the figure) is used to distribute the light over the visible portion of the screen for ordinary use.

The siren.—For certain purposes it may be desirable to get a key-note or a standard pitch from the tonoscope itself. The dots on the screen were therefore made as holes. At the base of the front of the case is a siren blow-pipe supported on a horizontal carrier so that it may glide freely over the surface of the drum, while a pointer indicates on the scale just what hole-frequency is blown. This makes it possible to produce as siren tones all the tones from 110 to 219 by one-vibration steps, excepting those rows of holes

which happen to be closed by the contact with the balance wheel. The siren blow-pipe is connected with a compressed air tank or it may be blown directly by a mouth tube. A speaking tube is used to carry the sound to the observer's ear and the opening and closing of this tube by means of a clamp starts and stops the sound.

The siren tone is not a tone of good quality. But a beautiful tone may be produced by projecting a beam of light through the holes in the screen upon a selenium cell in circuit with a telephone receiver. It so happens that the fluctuation in the resistance of the selenium cell takes approximately the form of a sine curve, and that produces a tone of most remarkably clear and smooth timbre in the receiver. One may, however, use any sort of instrument for giving the standard tone, as the pitch of the instrument can be read off on the tonoscope at any moment.

The reading.—Although the reading is simple and direct, it is necessary to mention some of the underlying principles. The first task is to see which row stands still, or the nearest still. This row indicates the desired record and will be seen irresistibly the moment the tone is produced, because all other rows are blurred or in rapid motion. Having identified the line which stands still we must next know within what octave the tone lies. If, for example, row 128 stands still this may represent a tone of 64, 128, 256, 512 vibrations, or even higher. Now from 110 v.d. to 219 v.d. the correspondence is direct and the dots appear as actually spaced on the drum within an octave above this, the dots double in number and therefore stand only half as far apart; and, within the second octave above, they quadruple in number. It is therefore easy to see instantly from the spacing of the dots within which octave one is reading. If the spaces in row 128, *e.g.,* are one-half of the original the tone is 256 v.d.; if they are only one-fourth of the original steps, the tone is 512 v.d., etc.

But in fine reading we deal with fractions of vibrations. If instead of one row standing still, two rows move slowly in the opposite direction, the tone lies between these, and the fraction is determined by the relative rate of movement of the two rows. In a very accurate recording of instruments this may be done to a high degree of certainly by timing the movement with a stop watch over a considerable period of time. To do this we observe how many dot spaces are moving up or down and apply this general rule: if the ascend-

ing row has been counted, *add* the fraction of dot space per second; or, if the descending row has been counted, *subtract* it from the integral number of the row observed. It is best to count the faster moving of the two rows.

Sources of error.—There can be no time-error in the transformation of the sound wave into an illumination wave; they must synchronize, since one is the direct cause of the other. There can be no progressive change in the speed of the motor because if the motor does not run in step with the fork it must stop. The only possible error on the physical side lies in a tendency to pendular oscillation of the drum which may show a tendency to "hunt" when starting or when the current is too weak. By allowing a minute for the "finding" immediately after starting, and by securing a right adjustment of the strength of the current, this hunting movement may be reduced to an inappreciable or negligible quantity. The presence or absence of this source of error may, of course, be ascertained at any moment by registering the tone from a standard fork as a control. The limit of accuracy in the registering of the apparatus is therefore set by the limit of constancy in the driving fork. This fork being carefully balanced, firmly mounted, and kept in fairly constant temperature, shows a very high degree of constancy and compares favorably with a standard 100 v.d. fork.

But the main source of error lies in the reading, particularly of high tones. This need, however, be a source of error only in rapid reading. By making the tone long enough to observe the rate of fractional movement, one may secure any desired degree of accuracy in reading, as, *e.g.*, in registering a tuning fork, by timing the fractional movements of the dots for a sufficiently long time. In brief, without giving numerical records, we may say that the limit of accuracy in the use of the instrument is really set by the limit of accuracy of a tuning fork, the driving fork. For actual tests of accuracy in reading see Miles (6).

The use of the tonoscope.—The tonoscope furnishes us the first ready and, at the same time, reliable means of measuring directly the pitch of a tone as sung, spoken, or played with a musical instrument. Heretofore, graphic recording has been the only reliable method. This has the merit of accuracy but is entirely too indirect and laborious to be of general use in practical work. As we have seen, it registers the tone as sung or played under natural

conditions, and the record is simultaneous with the tone. The scope of its usefulness is therefore very great. It furnishes us an approach to countless problems both in pure and applied psychology. The psychology of tonal expression is a field practically unworked as compared with the psychology of the appreciation of tone, largely because we have not before had any convenient means of measurement.

A few concrete illustrations from the laboratory may be cited. In standardizing the pitch discrimination test (9) it was found necessary to compare the relative reliability of available instruments. Tuning forks, string instruments, reed instruments, wind instruments, and sirens were all tested by direct registration upon the tonoscope. Temperature coefficients, air pressure coefficients, and resonance coefficients were worked out by the same mode of registration. While most of these measurements could have been made in other ways, the tonoscope proved at least a good, labor-saving device.

The settling of disputed questions of pitch has been interesting. For example, there was a pitch discrepancy in the playing of the oboe and the French horn in a symphony orchestra. Each player was given an opportunity to register a specific tone in the tonoscope, and it was found that the oboe was playing consistently 1.5 v.d. flat. A vocal soloist had a tendency to flat relatively high notes. She observed the error and learned to make the right correction. A singer was practicing to eliminate an undesirable fluctuation of the pitch of the voice and was much helped in practicing before the tonoscope as before a mirror. In a recent article (10) I have described some measurements which at the present time could be made successfully only with the tonoscope.

There is a conspicuous place for the tonoscope in the musical conservatory. The ear of the singer or player is too generous because it seldom has any objective correction. The pupil persists in constant errors because there is no objective check on the ear. But the tonoscope does for the ear what the microscope does for the eye. It magnifies and objectifies to the eye, bringing out even small details of the pitch of the tone.

An actual experiment in training of the voice by the use of the instrument revealed among other facts the following (5): A group of six singers practicing daily for twelve days, part of the time

with the instrument and part of the time without it, showed that the average result of training with the instrument was superior to the average result of training without it, by forty-two per cent. in the ability to strike a tone, by fifty-five per cent. in the ability to sing musical intervals, and by twenty-six per cent. in the voluntary control of the voice in sharping or flatting; and the ability gained by virtue of the aid of the instrument was transferred in large part to ordinary singing.

The entire article by Dr. Miles in the present volume (6) should be regarded as supplementary to this description and particularly as furnishing illustrations of the use of the instrument. The general trend of usefulness of the tonoscope in the psychological laboratory, the musical conservatory, and other situations in which the registration of the pitch of tones is desirable may be indicated in a partial outline of measurements which may be made with it, as follows:

I. Striking a tone.

1. Voluntary control of the pitch of a vocal or instrumental tone—the first and simplest test of ability to sound a given tone in true pitch.
2. The effect on the pitch of a tone of conditions varied under control; *e.g.*, the character of the standard tone, the absolute pitch, the mode of tone production, distraction, practice, seeing the registration when producing the tone, information about previous errors, deliberate correction, etc.
3. The pitch and reliability of the pitch of an instrument, as in tuning, testing, standardizing, and comparing instruments.

II. Sustaining a tone.

1. Degree of accuracy or ability in holding a tone.
2. Constant tendencies—sharp or flat.
3. Artistic effects in singing a "constant" tone, as in pitch tremulo.
4. Inflection of speech.

III. Minimal producible change.

A fundamental measure of discriminative action in the voluntary control of the pitch of a tone—a sort of psychophysic foot-rule which serves the same purpose on the motor side as the minimal producible change serves on the sensory side; *e.g.*, as a unit of measurement in the study of individual difference, or the effect of any other controlled variable.

IV. Tonal transition.

1. The mode and the accuracy of attack and release of tones in singing, in playing, or in inflection of speech (a) in measuring precision, (b) in registering some artistic effect, and (c) in musical or oratorical training.
2. Testing of instrument; *e.g.*, in proving that the piston pitch pipe is unfit for the sounding of a key-note because the note is necessarily attacked by a gradual sliding up to the key.

V. Tonal intervals.

Accuracy in producing musical intervals; *e.g.*, single steps, the natural scale, the chromatic scale, melody as in singing or playing of an air; (a) for the purpose of psychological or aesthetic study, or, (b) as a means of training in the musical conservatory.

VI. Transcribing of speech or musical records from the phonograph or any other recording instrument.

There are thus two quite distinct fields in which the tonoscope in its present form may be employed, namely, in the research laboratory and in the practical work of the musical conservatory.

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ACCURACY OF THE VOICE IN SIMPLE PITCH SINGING

BY

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Contents

The experiments here reported deal with two phases of simple pitch singing: (1) the ability of the voice to reproduce the pitch of a tone, and (2) the ability to make faint shadings in pitch, sharp or flat. The aim has been to formulate, if possible, a standard test for the measurement of each, to establish norms, and to investigate some of the underlying psychological factors.[1]

[1] The extensive measurements made would have been impossible were it not for the previous labor of Professor Seashore in perfecting a recording apparatus, the Tonoscope. Dr. Seashore has furthermore proved himself an unfailing source of inspiration and suggestion throughout the experimentation. The author is also under heavy obligations to Assistant Professor Mabel C. Williams, Dr. Thomas F. Vance, Messrs. Bruene and Malmberg, and the many observers for their kind and prolonged assistance.

HISTORICAL

The first investigator to employ the experimental method in attacking the problem of the accuracy of the voice in singing pitch was *Klünder* (11) 1872.[2] He used a manometric flame with two connected speaking tubes, an organ tone sounding in one while the observer sang simultaneously in the other. The difference in vibration number between the standard and sung tones was determined by counting waves. The average ± errors found on three tones, 128, 192, 256, v.d. are 0.761, 0.434, and 0.257 per cent. (of standards) respectively. The difference between 0.761 and 0.257 was thought to be due to the vocal cords and not to hearing.

Klünder was not satisfied with his method or his results and continued working on the problem, publishing a second time in 1879 (12). Again he used organ tones as standards and had his observers sing simultaneously with them, either in unison or in specified interval. The recording was done on smoked paper by means of two phonautographs. The two records were compared directly, that for the organ tone being used as a standard, and deviation in the pitch of the voice from that of the standard was computed in terms of .25 v.d. That Klünder was primarily interested in the physiological side of the problem is indicated by the questions which he set himself:

(1) Does our ear control the voice or is it controlled by the feeling of tension in the larynx? (2) How firmly does the voice attack tones? (3) Are the fluctuations of the voice such that give proof of control by the ear? (4) How many stress degrees of muscular tetanus are we justified in accepting through the performance of the muscles of the larynx?

Klünder found that for the pitches 96, 128, 192, 256, v.d., respectively, he himself as observer made the following ± errors: .32 v.d., .47 v.d., .62 v.d., and .59 v.d. This however was somewhat better than any of his other observers could do.

From this Klünder concludes that the voice is very accurate in reproduction of pitch and he answers his questions in substance as follows:

[2] Previous to this time Scott (17) and Blake (3) had developed phonautographs for registering voice curves.

(1) The vocal cords are held in labial tension by muscular tetanus. (2) The musculature allows from 40 to 170 different tensions in the tetanus. (3) The regulation of the pitch of the voice takes place directly through the sensation of tension in the larynx.

Seashore (19) in 1910 published in a very condensed form the results of experiments carried on in 1905 by himself and E. A. Jenner. Previous to that time, however, much work had been done in perfecting a registering apparatus, the tonoscope, which is fully described by Professor Seashore in the foregoing article in this volume of the Studies. Some preliminary experimenting also was done in 1901-'02 with the help of Edward Bechly, the results of which have never been published. Seashore and Jenner in their work sought to answer two questions: (1) Can we facilitate development of control in the pitch of the voice by using an aid to the ear in training? (2) May the ordinary limits of accuracy be exceeded by training with such an aid? In attacking these problems they used three measurements: (1) accuracy in reproducing a given tone, (2) accuracy in singing a required interval, and (3) the least producible change in the pitch of the voice. The standard or fundamental tone was 100 v.d., produced by a large tuning-fork; the intervals were the major third, the fifth, and the octave above this. The least producible change was determined for each of these four tones (1) in the least producible sharp and (2) in the least producible flat from the note as actually sung. Each period of practice consisted of one hundred and sixty trials, which took about forty-five minutes. The tests continued twelve days, approximately successive. During the first five days the singer depended entirely on the ear as in ordinary singing: then followed five days of singing with aid, *i.e.*, the observer was informed of the result of each trial immediately after it was made. The records of the eleventh day were taken without aid, while on the twelfth day aid was again given. Six men acted as observers. The conclusions of this investigation are quoted as follows:

"(1). The aid enhances the ability to strike a tone which has been heard. The superiority of the aided series over the unaided amounts to 42 per cent. (2) The aid enhances the ability to sing an interval. The superiority of the aided series over the unaided amounts to 50 per cent. for the major third, 50 per cent. for the fifth, and 60 per cent. for the octave. (3) The voluntary control

of the pitch of the voice is improved by the aid. The average superiority of the aided series over the unaided for all intervals amounts to 26 per cent. (4) There is probably some transfer of gain from the aided training to following unaided singing. (5) There is no evidence of transfer of the gain in the accuracy of the memory image. This is undoubtedly due to the fact we have here to do with memory rather than discrimination and the acquisition of accurate memory images is a slow process—too slow in this short series. (6) The gain in the discriminative control of pitch of the voice is fully transferred. (7) Improvements in the ability to sing a tone or an interval, and the ability to produce a minimal change, are very much more pronounced and more rapid in the aided than in the unaided series. (8) The second question is not answered absolutely by our records, but it seems probable (a) from the radical and immediate improvement of the aided series over the unaided, and, (b) from the introspection showing a tone which without the instrument seemed entirely satisfactory to the ear could be corrected by the ear after the error had been pointed out by the instrument, that a higher degree of accuracy of pitch in singing may be attained by aiding the ear in the training than would be possible to attain without such aid. No matter how keen the ear of a trained musician, it can be shown in a single test that his ear has been "too generous"—too easily satisfied, for when the error is pointed out objectively he can recognize it. We thus find cumulative evidence to show that the singer can not reach the physiological limit of accuracy by the ordinary methods of voice culture, because he has no objective criterion by which he can check up the accuracy of his ear. (9) The major third, the fifth, and the octave are approximately equally difficult intervals to sing. If we express the average error in relative fractions of a tone (1/25 of a tone) instead of in vibrations, the ratio is 1.4, 1.5, and 1.4, for the three intervals named above. The average error expressed in terms of vibrations shows that the difficulty of a natural interval varies approximately with the magnitude of the interval. (10) The minimal change is a relatively constant fraction of a tone within the octave. This is true for both the aided and the unaided series. If we reduce the records from vibrations to twenty-fifths of a tone, the minimal change is 3.1, 3.1, 3.6, 3.3, for the fundamental, the major third, the fifth, and the octave respectively. This is surprising,

because within this part of the tonal range the pitch discrimination is normally measured by a constant vibration frequency instead of by a constant part of a tone."

Cameron (4) 1907, varied somewhat the conditions of the experiment as performed by Klünder. In the first series the subject was asked to sing any tone of medium pitch, a second tone of low pitch, and a third of high pitch, and to sustain the pitch selected in each case as uniformly as possible throughout the singing. The second series was like the first except that each tone was interrupted by the insertion of short pauses of .3 second duration. In a third series, somewhat longer than those previously mentioned, the ability of one observer to imitate organ tones in the range 94 v.d. to 303 v.d. was tested. The tones were reproduced in sequence and, in chance order, partly simultaneously with the standards and partly by singing the tones immediately after the organ had ceased sounding. In a fourth series various distracting tones, (1) harmonious or inharmonious with the standard tone; (2) of greater or less interval from the standard; and (3) higher or lower than the standard, were introduced either at the beginning or just preceding the beginning of the reproduction by the observer. The more important results of the study are here summarized:

"(1) In the singing of a tone a sudden marked rise in pitch usually occurs near the beginning of the tone. This rise in pitch is so general as to seem to indicate a universal tendency. (2) No tone is sung entirely uniformly. It oscillates in pitch from period to period throughout its length in a somewhat irregular rhythmical fashion. (3) Very marked differences exist in different individuals with regard to their ability to imitate a standard tone. The subjects tested varied in degrees of accuracy in imitation of standard tones of different pitch from a small fraction of 1 per cent. to 13 per cent. of error. (4) There is manifest throughout a tendency to sing a tone higher than it should be sung. Thus the end of a tone is usually higher than the beginning and the sung tone (as a whole) is almost invariably higher than the standard tone. (5) Distractions when causing disturbances may affect the whole of the sung tone or only the beginning of the tone. In either case the effect of the distraction may be to cause the sung tone to vary from the standard (a) in the direction of the distracting tone; or (b) in the opposite direction from the distracting tone. (6) Sung tones vary-

ing from the standard under the effect or distraction are usually harmonious with the distracting tone. When the distracting tone is inharmonious with the standard tone, distraction is more likely to occur than when the two tones form a harmony. (7) A person may more or less closely imitate a tone which he has heard when his attention was engrossed in singing another tone of a standard pitch."

An important contribution to the general problem of control of the pitch of the voice in singing was made by *Berlage* (2) in 1910. During the summer of 1907 Berlage carried on a series of experiments in which definite time intervals were inserted between the breaking off of the standard tone and the beginning of the reproduction by the observer.[3] These intervals were of the following values stated in seconds: 1, 2, 3, 4, 5, 7, 10, 15, 20, 25, and 30. The tones were all sounded as *"a"* ('a' in 'ah'). This series is an amplification of the methods of Klünder and Cameron, and was undertaken for the purpose of finding the time interval most favorable for the imitation of tones, which when found became one of the conditions of further experimentation.

In the winter of 1907-'08 Berlage's general problem was to determine the influence of articulation and hearing in the vocal reproduction of tones. In this series (second) as in the third series by Berlage the standard tones to be imitated are voice tones. The variation of conditions consisted in having the standard tones sung part of the time by the observer and part of the time by the experimenter thus showing the immediate influence of hearing and of loud articulation in tone-reproduction. It seemed desirable to determine to what extent the influence of articulation is due to the larynx, and to the mouth cavity. For this purpose, in a third series of experiments, all the standard tones were sung by the observers, the vowel quality being varied under control. The standard and reproduction were sung, sometimes to the same vowel as *"i"*, *"i"*, or *"u"*, *"u"*, and at other times to different vowels as to *"i"*, and *"u"* or *"a"* and *"u"*. The chief conclusions reached from Berlage's experiments are the following:

(1) "Accuracy in the reproduction of a "strange" voice tone decreases rather regularly with the increasing time interval of from 1 to 30 seconds. Accuracy is greatest with an interval of from

[3] Berlage designates these tones as 'foretone' and 'aftertone'. 'Standard' and 'reproduction' are used throughout this study.

1 to 2 seconds. The values found here, for the variable average error, in the case of the observers amounted to only .5 v.d. and .6 v.d. (2) Observers reproduced their own voice tones more accurately than those of another (time interval 3 seconds). (3) The increase of precision shows itself chiefly in a decrease of the constant error. In the reproduction of outside standards and especially when they are near the boundaries of the voice range there is a tendency toward a constant error near the middle of the voice range. (4) In the reproduction of one's own tones vowel change works a disadvantage upon precision. With the standard tone sung as *"u"* and the reproduction as *"i"* there is a tendency for the latter to be lower, and vice versa when the vowels are changed. (5) In the reproduction of an outside standard the variable average error expressed in vibration frequency becomes larger with rising pitch, while if expressed in per cent. of vibration frequency it diminishes. (6) In the reproduction of one's own tones the variable average error expressed in vibration frequency remains rather constant with rising pitch. (7) The amount of departure of the individual tone sections (measured off in .1 second periods) from the general average of the reproduction shows no tendency, in the variations carried out in these experiments, to change according to the ordinal number of the tone sections in the course. (8) Only in the first .1 second is the reproduction regularly lower than the rest of the tone course. (9) Reproductions after the time intervals of from 3 to 10 seconds, in the case of two observers, show a sudden raising or lowering of the tone after the tone has progressed some .4 to 1.2 seconds. (10) The average departure of individual tone sections from the average for the tone is greatest in the reproduction of one's own tones. (11) The total amount of departure, expressed in vibration frequency grows with rising pitch so that—not considering rather marked irregularities with the individual observers—the amount of variation expressed in per cent. of a tone remains about constant."

The latest published study of this general problem to come to our attention is that of *Sokolowsky* (22) 1911. His apparatus consisted of a combination of the Einthovan string-galvanometer and the Weiss phonoscope. The organ tones, which were used for standards, acted on the string-galvanometer and the sung tones on the phonoscope. Both tones were registered in a convenient way for comparison by means of the Blix-Sandström photokymograph.

Sokolowsky secured the coöperation of seven professional opera singers, three men and four women. The observers were allowed to choose the vowel to which they sang the tones. The musical *"a"* was chosen most frequently. There were three short series of experiments: (1) singing a given tone simultaneously with the sounding of the tone by the organ (unison); (2) allowing a time interval between the organ tone and its reproduction. (The intervals used were 30, 60, and 120 seconds, during which the observers were instructed not to hum or sing to themselves); and (3) singing a specified interval from a simultaneously sounding organ tone. The musical intervals selected were the third, fourth, fifth, sixth and octave.

The results from these three series of experiments may be summarized as follows: (1) Curves for 8 tones were secured in series I. The average pitch was 251 v.d. (range 165 to 296 v.d.), the average error was ± 0.44 per cent. The average pitches for men and women respectively were 197 and 286 v.d., with average ± errors of 0.51 and 0.40 per cent. (2) The introduction of a time interval increases the average error to ± 0.99 per cent. as compared with ± 0.44 of the previous series. Errors are usually larger with an interval of 60 seconds than with 30 seconds. (3) The average error in series III is ± 1.51 per cent. The largest errors, average ± 3.28 per cent. are on the fifth, while the smallest, average ± 0.78 per cent. are on the third. (4) Of the entire number of tones counted (46) 36 are sung flat and 10 sharp. The errors on the side of sharping are divided among three women and one man; those on the side of flatting between three men and three women.

Guttmann (6) 1912, in his consideration of the psychophysics of singing gives some attention to the problem of accuracy in reproducing pitch and states that recently he has been engaged in an extensive research in this field. The results are to be published shortly in one of the psychological journals, but in a preliminary way he says that they agree in general with those secured by Klünder and Sokolowsky, but he thinks that the results of the latter (unison curves) are "too good".

Other investigators, among them *Hensen* (10) and more recently *Marbe* (14), *Grützner* (5) and *Scripture* (18) have developed methods for recording the pitch of the voice, but these seem not to have been used in gathering data on our problem.

THE TONOSCOPE

In the investigations of Klünder, Cameron and Berlage the vibration frequency of the tones was recorded in tracings on smoked paper. Sokolowsky photographed his records; after these had been rendered permanent the waves were counted and the pitch determined by comparison with a time or standard line. This method, commonly known as "graphic recording" has been used with various refinements by many investigators in the field of phonetics. While reliable, it is at best indirect and very laborious.

Seashore and Jenner in their research made use of an early model (20) of the tonoscope. This instrument as lately improved was used by the author in the present experiments.[4] It has several advantages which recommended it as an instrument for the measurement of the pitch of tones. In the first place readings are made quickly and directly. The instant a tone is sounded the vibration frequency is indicated by a row of dots. The experimenter has simply to note the number of this row and to record it. He is, therefore, enabled to secure a large number of observations in a relatively short time. It is not difficult to take two hundred records in thirty minutes. In the second place the experimenter has the advantage of knowing how the test is progressing. If a preliminary practice series is desired to acquaint the observer with some procedure we have in the direct readings from the tonoscope an index to the observer's understanding of the test. The observer must be kept actively trying throughout the experiment. In psychological tests, such as the imitation of tones by singing, there is so much repetition in the program for the observer that his attention easily wanders. Large and unnatural errors are therefore likely to be found in the records. Here the tonoscope as a recording instrument has an advantage over other methods as it provides for detecting these errors as soon as they occur. The experimenter as he takes each reading notes roughly the attack, the steadiness, and the degree of success with which the reproduction approaches the standard. He thus easily becomes acquainted with the unusual range of variability and forms an estimate of the observer's power to control his voice. When a

[4] The instrument is fully described in the preceding article in this volume of Studies, "The Tonoscope", by Professor Seashore. A reading of that article is essential for an understanding of the present report.

tone of unusual divirgence is given he therefore immediately recognizes it and can take cognizance of it, asking for introspection or for a new trial, and all with scarcely any loss of time. He may thus check up and to some extent control the observer,—keep him at his best. Furthermore the possibility of encouraging the observer or even of giving him full information regarding the success or failure of each trial is in itself a most important asset.

The tonoscope has been criticised as giving only an approximate result, because the pitch of the singing voice is not uniform and it is therefore necessary in reading the instrument to select the predominating pitch. This criticism stands or falls according to the needs of the problem to be attacked. If one were studying the oscillations of the voice, or the variations of the individual sections of a tone, as for example the difference in pitch between the first tenth and the fifth tenth of a second of a tone, it would be better to use a graphic method. But even in such problems as these the tonoscope is not without its possibilities. The characteristics of tonal attack in singing are easily discernible in the configurations on the screen. With many of the problems which lie in our field there is no need for so detailed a record. The predominant or modal pitch of a tone of from one to two seconds in length is all that is needed for much of the work in the psychology of pitch singing. The tonoscope can of course meet this condition admirably, as it is this modal pitch which stands out clear and distinct, forcing itself upon the attention of the experimenter.

Tonoscope reading test.—The method of reading the tonoscope, and the various sources of error having been fully treated by Professor Seashore in the accompanying article, there is no need to repeat them here.

In order to determine the degree of accuracy in the reading of the tonoscope the following experiment was performed. A set of ten large, movable-disc, tuning forks ranging from 128 to 131 v.d. was so tuned that no two forks had a pitch difference of over 3 v.d. and in the great majority of cases the differences were much smaller. A revolving shutter, rotated by the tonoscope shaft, was so arranged as to expose the mouth of a resonator connected with the sensitive light for the following time intervals: .25, .50, .75 and 1.00 second. In this way a tone sounded before the shutter was registered by the tonoscope for just the period during which the

resonator was exposed.[5] The presentation of the tones and the recording of the observations were in charge of two helpers. The experimenter did nothing but watch the moving screen and call out the readings. He had no way of knowing the real reading in any case. Five trials were given on each fork with each exposure interval. The order of the forks was determined approximately by chance. There was an interval of about five seconds between tones.

After the fifty trials with the .75 second exposures were finished, the pitch of each fork was carefully determined with the tonoscope, counts being made by the stop-watch during periods of from 6 to 15 seconds. These records formed the basis from which to compute the errors in the first test. The assistant then changed the pitch of all the forks and the above procedure was repeated with a .50 second exposure. Again the forks were changed and the same procedure was followed for the 1.00 second and the .25 second exposures in turn. Thus fifty records were obtained for each of the four exposure periods and the conditions were such that the reader could have no accessory clue. The record is summarized in Table I.

To test the reading ability for tones one octave higher, *i.e.* 256 v.d., where it will be recalled the tonoscope reading, and hence the errors, must be doubled, a set of seven small forks was provided. These were weighted so that no pitch difference between any two forks was greater than 3 v.d. The test was made with the exposure interval of .75 second.

In making the pitch difference between the forks come within a range of 3 v.d. we approximate the condition presented when working with voice tones that require accuracy in reading. If an observer is asked to reproduce a tone or to sing an interval the experimenter knows approximately the point on the scale where the reading should occur. He is watching this point. Should the reproduction be nearly correct and the tone fairly constant for, say .50 second, he can read according to our result (see Table I) within an error of less than $\pm$ .2 v.d. If however the reproduction goes wide of the mark, for example to the extent of 6 v.d. there is no need of reading in fractions smaller than halves.

[5] This arrangement is not ideal in that, as the tone is turned on and cut off by the disc, slightly disturbing waves are set up and show on the screen. In test No. 4 where the tone sounded for .25 second this was felt to be very disturbing. The real time given for the reading of the tones in all these tests was thus slightly less than that represented by the several discs.

TABLE I. The degree of accuracy in the reading of the tonoscope

Exposure 1.00 sec.		Ave. error	.12	v.d.	; m.v.	.10	v.d.
" .75 "	(128 v.d.)	" "	.15	"	; "	.12	"
" .75 "	(256 v.d.)	" "	.19	"	; "	.17	"
" .50 "	(128 v.d.)	" "	.18	"	; "	.11	"
" .25 "	(128 v.d.)	" "	.65	"	; "	.27	"

EXPERIMENTS SERIES I: ACCURACY AND THE VOICE RANGE

In the first five series of experiments the purpose was to answer questions concerning some factors which must be considered in any adequate test of voice control. (1) How does accuracy of control vary with the range of the voice? (2) How does the intensity of the standard tone affect the pitch reproduction? (3) What is the relation of voice volume to voice control? (4) Are the reproductions affected by the timbre of the standard tones? (5) Do vowel changes (timbre changes) in the reproductions cause changes in the pitch of the reproduction? The sixth series represents an effort to combine into a single test the results of our previous experiments, together with those of other investigators, and to give this test to a sufficiently large group that we might be enabled to determine from the results some of the norms of voice control.[6]

Seventeen men with splendid enthusiasm gave their services as observers in the experiments of Series I. From among this number several were selected to serve as observers in Series II, III, IV, and V. The observers were all of mature age and more than half their number had had some training in the methods of experimental psychology. P, the only professional musician in the group, is a teacher of "Voice" and a thoroughly trained tenor soloist. H, a baritone of extensive special training, has for some time been the leader of a large choir. He is a soloist of ability. Ma., W, and V. Z. have all had special training in singing, and much experience in solo, quartette and glee club work. S, C. Mi., Ro., An., Wi., and V. H. all have had considerable experience in general singing but are without special training. Ri., Ab., Mc., Br., D, and Bh. very seldom sing in public but they enjoy music.

[6] Gutzmann (7) and Sokolowsky (22) suggest some of the above problems, especially Nos. 1 and 5 as being important. These articles and suggestions however did not come to the attention of the writer until the experimentation was completed.

For Series I the standard tones were provided by a set of twenty tuning forks ranging approximately by the chromatic scale from C_1, 64 v.d., to and including a′, 426 v.d. The first fourteen forks beginning with 64 v.d. were large and carried discs. All the tones were of good quality and their duration of tone was more than ample. Some of the forks were of different vibration frequency than that indicated by the notes of the chromatic scale; for example, the pitch of the fork that corresponded to G was 182 v.d. in place of 192 v.d. These differences were made in order to check the observers from judging and singing the various steps as musical intervals.

An independent selection of five forks was made for each observer after a preliminary determination of his voice range. These forks covered approximately one and one-half octaves in the middle of the range and were fairly distributed. In giving the test the experimenter presented the tones to the ear of the observer, who, after listening for 1.5 seconds and allowing a time interval of 1 second, reproduced the pitch of the tones as accurately as possible. Proceeding from the lowest to the highest and then in reverse order back to the lowest, each tone was given twice in succession, the test consisting of twenty trials on each standard tone.

The results of these experiments are present in Table II. O denotes the observer; P, the pitch of the standard tone; E, average error; m.v., mean variation; and C.E., constant error. These five successive columns give the record of the respective standards for each observer. The footings in the table show the averages of the figures above stated, first in terms of vibration (absolute) and second, in terms of percentage of a tone (relative) at the respective levels. The average C. E. in the footing is the average of C. E. regardless of sign; in the second the sign is taken into account giving group tendency of the constant error, or group constant error (G. C. E.). These footings are represented graphically in Fig. 1.

Taken as a whole these records show that accuracy in the reproduction of the pitch of a tone, as measured by the average error (E) with its mean variation (m.v.) the average of the constant errors (C. E.) and the general tendency of the constant errors (G. C. E.), tends to be a constant in terms of vibration frequency. This is shown in Fig. 1 (A) by the fact that the four curves, for

the absolute variation tend to remain horizontal lines whereas the four curves for the relative variation (B) tend to fall in inverse ratio to the rise of the pitch. The slight tendency to deviate from the constant in terms of vibrations is in the direction of decrease in accuracy with rising pitch. This, in the case of the highest tone,

TABLE II. Accuracy and the voice range

O	P	E. m.v. C.E.	P	E. m.v. C.E.	P	E. m.v. C.E.	P	E. m.v. C.E.	P	E. m.v. C.E.
P.	128	1.6 1.1 —1.5	160	1.2 .6 —1.2	182	.9 .8 + .4	256	1.9 1.6 —1.6	320	1.2 1.2 + .4
H.	95	1.4 .6 +1.4	120	3.1 1.1 +3.1	160	.7 .7 — .1	213	2.9 1.4 —2.9	286	3.7 1.4 —3.7
Ma.	95	1.5 .7 —1.5	120	.8 .8 + .4	160	3.0 1.1 +2.9	240	3.7 1.0 +3.7	286	3.1 1.3 +3.0
W.	120	.9 .9 + .2	144	1.0 .8 + .4	182	1.7 1.6 — .2	256	2.5 1.5 +1.9	320	1.8 1.7 — .1
V.Z.	120	3.1 1.1 +3.1	144	1.6 .7 +1.6	182	1.7 .9 +1.4	256	1.1 1.1 + .3	341	2.7 1.4 +1.7
S.	120	2.6 .8 —2.4	144	1.8 .9 —1.5	182	1.0 .8 — .3	240	2.1 1.2 — .9	286	7.1 1.8 +7.1
C.	95	.9 1.2 + .2	144	.7 .7 — .0	182	.8 .6 — .3	256	1.3 1.9 + .8	341	1.7 1.5 — .9
Mi.	86	2.9 2.3 +4.0	120	1.1 .8 +1.0	182	1.6 1.9 — .1	256	2.7 2.7 +1.0	341	3.5 1.8 +3.4
Ro.	95	2.3 4.5 + .1	144	2.9 2.2 +2.5	182	2.1 2.0 +1.3	240	4.5 1.8 +4.3	320	2.5 2.0 + .2
An.	95	5.0 2.3 +3.7	120	1.6 1.5 + .8	160	2.5 2.8 —1.0	213	4.5 2.6 —4.1	256	4.6 3.5 —3.1
V.H.	107	3.5 1.7 +2.7	128	4.2 1.4 —3.6	160	2.0 1.7 —1.0	182	1.2 1.0 + .1	240	4.2 2.2 +3.9
Ri.	95	2.3 1.9 +2.5	144	2.9 2.3 —1.6	182	2.1 1.9 + .2	240	4.5 1.5 — .5	320	2.5 2.1 +2.7

TABLE II. (*Continued*)

		3.2		5.5		3.1		1.3		1.4
Ab.	95	1.0	128	1.4	160	.9	182	.8	256	.9
		+3.1		+5.5		+3.1		+1.7		+ .9
		1.7		3.9		1.9		1.0		2.6
Mc.	95	.5	120	1.3	160	.8	213	.9	256	1.2
		—1.6		+3.8		—1.7		+ .4		+2.0
		2.9		5.6		6.4		3.7		2.7
Br.	95	2.2	144	1.0	182	1.1	256	1.0	384	2.8
		+2.7		+5.2		+6.4		+3.6		+1.3
		1.7		3.3		5.9		6.3		11.2
D.	95	.9	128	3.1	160	2.4	182	6.6	256	5.4
		—1.4		— .3		—5.9		—3.4		—6.5
		1.6		2.4		3.0		5.1		2.5
Bh.	120	1.1	144	1.6	182	1.1	240	1.8	286	1.4
		—1.3		+ .9		+3.0		+5.1		+1.5
P	103.0		135.0		172.9		230.6		299.4	
Average in v.d. E		2.3		2.6		2.4		2.8		3.5
m.v.		1.5		1.3		1.3		1.8		2.0
C.E.		2.0		2.0		1.7		2.1		2.5
G.C.E.		+ .8		+1.0		+ .5		+ .5		+ .8
Ave. in per cent. of a tone E		.18		.15		.11		.09		.10
m.v.		.11		.08		.06		.06		.05
C.E.		.15		.12		.08		.07		.06
G.C.E.		+.06		+.06		+.02		+.02		+.02

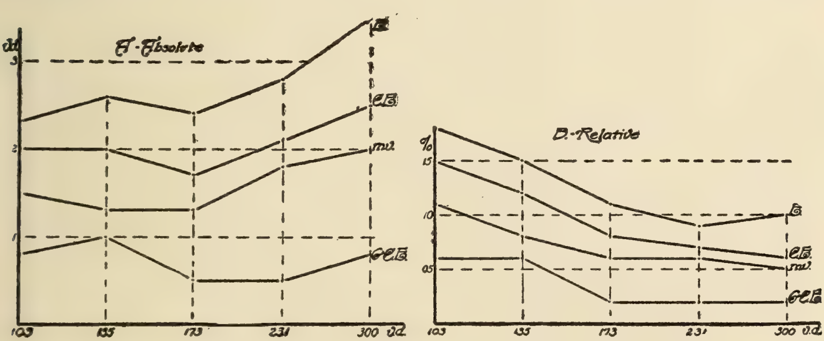

Fig. 1. Accuracy and the voice range. (Table II)

is to be accounted for mainly by the fact that some observers were erratic on this tone, probably because the tone was higher than the observer commonly sings. As a matter of fact only half of the observers, nearly all of whom would be classed as bass or baritone in their range, show any tendency to decrease in accuracy at this

point; five show the tendency to increase in accuracy and the remaining four tend to remain constant.

This result is in harmony with results found by *Preyer* (16), *Luft* (13), *Meyer* (15) and *Vance* (26) on the sensory side, that pitch discrimination is approximately constant in terms of vibration frequency within this range. It is in harmony with the finding of Berlage (2) as quoted above: item 5 (second part), that average error diminishes with rising pitch if expressed in per cent. of vibration frequency; and item 6, with reference to the reproduction of one's own tones.

It is interesting to compare and to contrast these records with those of Seashore and Jenner (19), item 9, showing that the average error in the singing of a natural interval (third, fifth, and octave) varies approximately with the magnitude of the interval; (see also Sokolowsky's results above) and, item 10, showing that the minimal change is a relatively constant fraction of a tone within the octave.

The tendency for the C. E. to be in one direction (+) will be considered in a later section in connection with the constant errors in our other series of experiments.

EXPERIMENTS SERIES II: INTENSITY OF STANDARD

In the experiments of Series I, as stated above, two successive trials were made on each fork. Occasionally upon the presentation of the tones for the second trial at reproduction the observer would say "Let me hear that again; it sounds higher (or lower) than before", or "Is that the same fork?" Such remarks by careful observers led to this consideration of the intensity factor.

The same forks were used with the respective observers as in Series I. The tones were presented to the ear by the experimenter as before. But with half of the trials the standards were made about as strong as possible by striking the forks a heavy blow and presenting them near the ear. The other standards were made as weak as could be heard with distinctness. The observers were encouraged to sing with a medium volume of voice and not to imitate that of the forks, as is the natural tendency. Twenty records were made with each fork, ten on the "weak" and ten on the "strong" in the double fatigue order, as regards pitch and intensity. No successive trials were made on the same fork except on the highest and lowest. Having sung the tones from the highest the

observer would sing them in reverse order from highest to lowest; but a short pause was introduced between such successive reproductions. Of the eight observers tested, P, Ma., V. Z., C, An., V.H., S and Mi., the first six had no definite knowledge of the object in view.

The results are shown in Table III and graphically represented in Fig. 2. *"W"* denotes weak and *"S"* strong, while the other notation is the same as that previously used. It will be seen that the intensity of the standard tone has a decided effect upon the accuracy of reproduction.

(1). Increase in intensity causes a lowering in the pitch of the reproduction. The G. C. E. for S on each of the five levels is less than that measure for *W.*, the minimum amount of difference being 1.4 v.d., the maximum 4.1 v.d. and the average for the five pitches, 2.3 v.d. In all the forty individual constant errors with the exception of two (see V. H.'s lowest tone and C.'s highest; in this latter *"W."* and *"S."* are just the same) the reproductions of the "strong" standards are lower than those of the "weak". If we compare these averages (C. E.'s and G. C. E.'s) with those of the previous series of experiments we find not only that the "strong" C. E.'s and G. C. E.'s are lower in the majority of cases than those of Series I, but that these measures for the reproductions made from the "weak" standards are somewhat higher than those of the former series. The effect of intensity in other words, is evident in both "weak" and "strong" standards, the former heightening the seeming natural tendency to sharp and the latter overcoming this tendency with a more powerful one to flat.[7]

(2). Strong standard tones cause general inaccuracy of voice control. Most of the observers stated that they were less sure with the "strong" standards. Others complained that the test made their ears tired. Reference to the mean variations and also to the E.'s and C. E.'s will show that in the majority of cases these amounts are

[7] When the conditions of this experiment (Series II) were explained to P., the professional musician, he remarked off hand as he began the test: "Loud tones would make your nerves more tense and would in general tend to make you sharp." He was asked then and at other times during the test to let any conscious tendency to flat or sharp take care of itself *e.g.* not knowingly to correct for it. At the last P. said: "I am equally satisfied with my reproductions of weak and strong." Cf. P's. record in Table III.

TABLE III. *Intensity of standard tones*

Ave. P.	105 v.d.		135 v.d.		174 v.d.		237 v.d.		301 v.d.	
	W.	S.	W.	S.	W.	S.	W.	S.	W.	S.
	E.	E.	E.	E.	E.	E.	E.	E.	E.	E.
	m.v.	m.v.	m.v.	m.v.	m.v.	m.v.	m.v.	m.v.	m.v.	m.v.
O.	C.E.	C.E.	C.E.	C.E.	C.E.	C.E.	C.E.	C.E.	C.E.	C.E.
	1.3	2.1	.7	1.2	2.3	1.4	1.1	.7	3.0	1.0
P.	1.0	.6	.5	.9	1.2	1.4	.7	.7	1.3	.8
	— .7	—2.1	+ .3	—1.0	+2.3	+ .3	+1.1	+ .2	+2.9	+ .5
	1.1	1.1	1.0	1.4	3.2	1.7	3.1	2.1	2.1	1.4
Ma.	.9	.9	1.0	.8	.8	1.7	.6	1.0	.7	1.0
	+1.0	— .7	— .2	—1.4	+3.2	+ .8	+3.1	+2.0	+2.1	—1.4
	3.8	1.6	2.9	1.1	3.2	.7	1.9	4.8	2.0	1.8
V.Z.	.7	.9	1.3	.7	.5	.7	1.5	1.2	1.7	1.3
	+3.8	+1.2	+3.1	+1.0	+3.2	— .2	—1.2	—4.8	+ .3	—2.1
	1.2	.7	1.9	1.1	1.7	.7	3.0	2.4	2.8	2.6
C.	.5	.7	.5	1.1	.8	.7	1.1	.9	2.0	1.0
	+1.2	+ .2	+1.9	+ .3	+1.7	— .1	+3.0	+2.4	+2.6	+2.6
	6.4	5.4	1.9	1.1	1.9	2.3	3.2	7.8	3.7	8.1
An.	6.4	2.8	1.3	1.1	1.9	1.3	1.2	1.7	1.8	1.2
	+6.4	+4.8	+1.9	— .1	— .5	—1.9	—3.1	—7.8	—3.4	—8.1
	3.1	2.9	3.9	3.5	1.0	1.9	2.2	2.1	2.9	8.0
V.H.	1.6	1.7	1.0	1.5	.8	1.8	1.9	2.1	3.0	3.4
	+2.6	+2.9	+3.9	+2.9	— .7	—1.2	+ .8	+ .3	— .5	—8.0
	1.8	4.2	1.0	.7	.8	3.6	2.1	4.7	5.3	5.5
S.	.5	.7	.5	.6	.8	1.0	1.2	.9	1.3	1.4
	—1.8	—4.2	+1.0	— .4	— .2	—3.6	—1.8	—4.4	+5.3	+5.5
	9.3	8.5	1.5	1.7	2.2	5.4	4.0	4.5	7.8	4.7
Mi.	1.9	4.1	.9	.9	2.0	2.9	3.1	4.1	1.9	2.9
	+9.3	+8.5	+1.5	+ .5	— .8	—5.4	+2.4	—2.1	+7.8	—4.6
Av. E.	3.5	3.3	1.9	1.5	2.0	2.2	2.6	3.6	3.7	4.1
Av. m.v.	1.7	1.6	.9	1.0	1.1	1.4	1.4	1.6	1.7	1.6
Av. C.E.	3.4	3.1	1.7	1.0	1.6	1.7	2.1	3.0	3.1	4.1
G.C.E.	+2.7	+1.3	+1.7	+ .3	+1.0	—1.4	— .5	—1.8	+2.1	—2.0

larger with strong standards, thus indicating conditions that operate against the best vocal control.

The matter of intensity has been considered in the field of pitch discrimination, where it must really be worked out. Seashore (21) makes the following statements concerning it:

"Extensive experiments show (1) that both trained and untrained observers may be influenced by intensity in their pitch judgment; (2) that although there is a tendency among the untrained, especially the ignorant, to judge the loud tone the higher, it may work either way; (3) that the same individual may show one tendency at one time and the reverse at another; (4) that for trained observers the two tendencies are about equal; and (5) that the tendency is more serious for large than for small intensity differences. Introspection shows that this confusion rests largely on motor tendencies, or motor images. We associate high and strong with strain—the reversal can in some cases be traced to a correction, conscious or unconscious, based on knowledge of this danger.

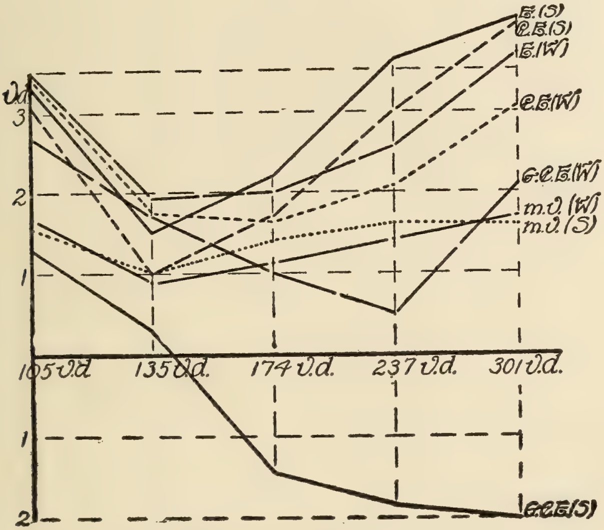

Fig. 2. The influence of intensity of standard tones.

Experiments show that the just perfectly clearly perceptible tone is most favorable for accurate results. It is ordinarily purer than a stronger tone and favors concentration. Experimenters must guard against a very common tendency, usually unconscious, to facilitate the discrimination by making the tones loud; and untrained observers usually desire (unwisely) a loud tone."

These conclusions are found on tests made by Anderson (1) at the level of 435 v.d. Our results just stated led to a re-examination of the effect of intensity on pitch. Hancock (8) found that as

measured in terms of hearing alone the tendency to hear a relatively low strong tone as low is greater than is shown in this series for singing. All these facts make clear that in singing from a standard tone greater care must be exercised to keep the tone constant and at a most favorable strength. We have no adequate quantitative data to show what strength is best but the facts available tend to support the statement made by Seashore (21) that the just perfectly clearly perceptible tone is most favorable for accurate results.[8]

EXPERIMENTS SERIES III: VOLUME OF THE VOICE

The effect produced by varying the intensity of the standard tones suggested a parallel question concerning the relationship of voice volume and accuracy of reproduction. This problem was attacked in the following manner. The forks selected were the same for each observer as in the voice range test, Series I; they were presented to the observer's ear by the experimenter who endeavored to keep the intensity as nearly constant as possible, and the observer was instructed to reproduce the tones in three degrees of voice volume, "loud", "medium" and "weak". Ten trials were made on each fork with each of these three degrees of loudness of voice, the order being as follows: one trial on each fork from lowest to highest and after a pause from highest to lowest with "medium" intensity; from highest to lowest and back to highest with "loud" intensity; from lowest to highest and back to lowest on "weak"; highest to lowest and back on "medium" and so forth until the 150 trials were made.

These records are summarized in Table IV and represented in part in Fig. 3. In this table *L, M* and *W* represent respectively loud, medium, and weak, other notation is the same as in the foregoing tables.

Here we find again, as in the foregoing series, the tendency for accuracy in singing to remain a constant in terms of vibrations, except for the extreme notes, at which there is a decrease in efficiency, especially at the high note. The form of the average error curve (E)

[8] The force of the blow changes the pitch of a fork, (See Winkelmann's Akustik Vol. 2 p. 358) lowering it slightly, but this change in these forks could hardly be detected and certainly fails to acccount for the error in reproduction. See also Seashore (21).

here is entirely analogous to the form of the curve of pitch discrimination referred to above (16, 13, 15 and 26) but it represents a shorter range, as the voice has a shorter range than the ear.

The constant error for men here, as in the foregoing series, is in the direction of sharping. It is a relatively constant fraction of a vibration for all pitches except the highest. The records for the medium and the weak tones practically coincide,[9] and compare very favorably with those of Series I., but there is a uniform tendency to sing the loud reproduction highest. The average difference between the loud and the weak (see the G. C. E.'s) is here .6 v.d. This is not a contradiction, but the reciprocal of the results found in Series II: namely, that the loud (or strong) standard is reproduced low.

It will be remembered that in Series II the standard was made strong, the observer tried to produce a tone that subjectively seemed the same in pitch, and that practically all of his reproductions were flat. This result in the light of Series I, where sharping was the rule, seemed to warrant the conclusion that the strong tone is judged low. Now in Series III we have a confirmation of this; here the standard is of medium intensity while the reproductions are varied: loud, medium and weak. It seems therefore that the instant the observer commences his loud reproduction he is subject to the same error in judgment as was revealed in Series II, and that to make his reproduction subjectively equal in pitch to the standard, he thinks it necessary to raise. This brings about abnormal sharping: the average G. C. E. of Series III is +1.3 v.d. as against +.7 v.d. for Series I, where intensity differences were at a minimum.

The agreement of the errors (G. C. E.'s) in these two series (II and III) at once offers an explanation for them: the error is primarily one of hearing which is basal and the chief cause for the error in singing.[10] This is in harmony with the contention of Klünder

[9] Medium and weak tended to be confused by the observers who would frequently have to be reminded that they were not making sufficient difference between them. This would imply that they each seemed more natural and less distinct than the loud, which is borne out by the fact that the average G. C. E. for weak (Series III) is identical with that for Series I, *i.e.* +.7 v.d. However it should be noted that the curves for weak are less regular than in Series I.

[10] It is interesting to note than in Series II the high strong is flattened most, while in Series III the high loud is sharped most.

TABLE IV. *Volume of the voice and accuracy*

Ave. P.		109 v.d.			137 v.d.			176 v.d.			241 v.d.			308 v.d.		
		L	M	W	L	M	W	L	M	W	L	M	W	L	M	W
O.	E. m.v. C.E.															
P.	E.	3.0	2.0	1.3	2.8	1.4	.9	[illegible]	.9	1.8	1.9	1.3	1.5	1.2	2.7	4.1
	m.v.	.6	.8	.7	.5	1.2	.5	.8	.9	.9	1.1	1.3	.6	1.1	1.6	.8
	C.E.	—3.1	—1.9	—1.2	—2.7	— .9	+ .1	— .3	— .2	+1.6	—1.7	— .5	+1.5	+ .8	+2.4	+1.4
Ma.	E.	2.0	1.8	1.3	2.0	1.4	1.7	2.5	3.0	3.9	2.2	1.0	1.1	1.4	1.7	2.4
	m.v.	.7	1.0	.5	1.6	1.4	1.5	1.3	1.3	.9	1.0	1.0	1.1	1.2	1.5	1.3
	C.E.	—2.0	—1.5	—1.3	—1.5	+ .1	— .5	+2.4	+3.0	+3.9	—2.2	+ .5	+1.1	—1.2	—1.2	+2.3
V.Z.	E.	3.9	3.5	3.2	2.9	2.5	2.6	2.3	2.0	1.3	2.2	2.5	2.1	1.0	1.2	1.3
	m.v.	1.0	1.1	1.4	1.1	1.2	1.4	1.6	1.9	.7	1.2	1.0	.7	1.0	1.2	1.0
	C.E.	+3.9	+3.5	+3.2	+2.8	+2.5	+2.3	+2.0	+1.8	+1.0	—1.7	+2.3	—2.0	— .5	— .4	+1.2
S.	E.	2.3	2.3	2.7	1.2	.9	1.1	3.2	2.4	2.6	.9	1.4	.9	7.6	5.2	5.2
	m.v.	.8	.4	.5	1.2	.8	1.0	1.1	.6	.7	.8	.9	.8	1.9	1.4	1.5
	C.E.	—2.2	—2.2	—2.6	+ .1	— .5	+ .6	+3.1	—2.4	—2.6	+4.4	—1.0	— .3	+7.5	+5.2	+5.2
C.	E.	1.4	1.2	1.0	1.9	1.4	1.7	1.5	.4	.7	3.0	3.6	2.4	5.6	3.9	2.8
	m.v.	.5	.6	1.0	1.1	.6	.8	1.5	.4	.8	.9	.4	1.1	1.5	1.6	1.1
	C.E.	+1.4	+1.2	— .6	+1.3	+1.4	+1.7	— .5	— .4	+ .4	+3.0	+3.5	+2.3	+5.5	+3.8	+2.8
V.H.	E.	9.0	4.9	2.5	2.1	1.7	1.6	2.9	2.2	4.3	1.6	2.2	2.0	2.2	4.2	3.0
	m.v.	1.8	2.1	1.3	1.4	1.5	1.6	2.3	2.0	3.6	1.7	2.8	2.0	1.5	2.4	2.8
	C.E.	+8.9	+4.9	+2.5	+2.1	— .8	+2.0	—2.1	—1.2	—2.5	+ .4	+ .8	— .4	—1.3	—4.2	—2.0
Mi.	E.	2.3	1.6	1.7	2.7	2.6	2.7	3.1	2.3	2.0	3.1	1.3	2.6	9.5	3.5	3.0
	m.v.	1.6	1.5	1.2	1.5	1.6	2.0	2.4	2.3	1.6	2.3	1.4	2.2	3.5	1.0	2.6
	C.E.	+2.2	+1.2	[illegible] 1.3	+2.7	+1.9	+2.3	+1.5	+ .3	—1.2	+3.1	+ .7	+1.8	+9.5	+3.5	+1.2
Av. E.		3.4	2.5	2.0	2.2	1.7	1.8	2.3	1.9	2.4	2.1	1.9	1.8	4.1	3.2	3.1
Av. m.v.		1.0	1.1	.9	1.2	1.2	1.3	1.6	1.3	1.3	1.3	1.3	1.2	1.7	1.5	1.6
Av. C.E.		3.4	2.3	1.8	1.9	1.2	1.3	1.7	1.3	1.9	2.3	1.3	1.3	3.8	3.0	2.3
C.G.E.		+1.3	+ .7	+ .2	+ .7	+ .5	+1.0	+ .9	+ .1	+ .1	+ .8	+ .9	+ .6	+2.9	+1.3	+1.7

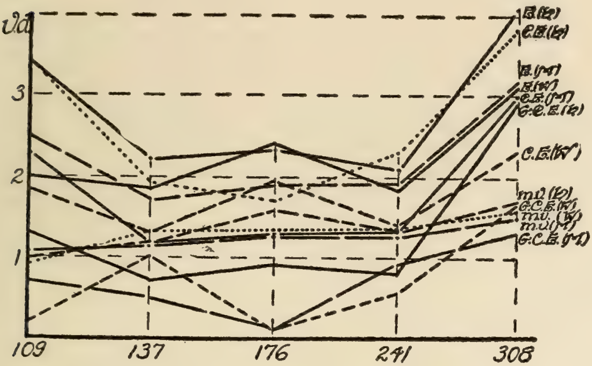

Fig. 3. Volume of the voice and accuracy (Table IV).

(12) that the ear is the chief criterion for regulating the voice. But the result quoted from Hancock (8): that the hearing error is greater than the singing error, (when dealing with a low, strong tone), together with the fact (Series III) that there is relatively more flatting with a strong standard than there is sharping with a loud reproduction, would conform to the conclusion reached by Stern (24) that the kinaesthetic sense of the singer is also an important factor.

One would expect a larger mean variation (m.v.) for the tone that has the largest error, but the table shows the mean variation to be practically equal for all three intensities of sound. This may be taken as a mark of the relative constancy of the motive for the intensity error.

The agreement and the remarkable uniformity in these two laws as shown in Series II and III would indicate that we are here dealing with an important factor of which we must take cognizance, both in the hearing and the producing of musical tones.

EXPERIMENTS SERIES IV: TIMBRE OF STANDARD TONES

Klünder (11 and 12), Cameron (4) and Sokolowsky (22) in their researches used organ tones for standards, while Berlage (2) made use of tones from the voice. Having ourselves used tuning forks it seemed advisable to ascertain if timbre differences in the standards affect the accuracy of reproduction.

The standards selected for the test were: a large disc tuning fork (144 v.d.) sounded before a resonator, the dichord (137.5 v.d.) energized by bowing, and an organ pipe blown by mouth. In using the latter, because of the variability of the blow and hence the uncertainty of the pitch sounded, the vibration frequency of each standard tone was read on the tonoscope and entered in a parallel column with the reproductions. The tones were so far as possible of uniform intensity, they were sounded for approximately 2 seconds and after the interval of 1 second reproduced on *a*, as in "law" with medium volume of voice. Twenty trials were made on each standard, and because the effect of timbre was the point of interest, the reproductions were in groups of five successive trials, the standard of course being sounded before each attempt.

TABLE V. Timbre of standard tones and accuracy

	Fork 144 v.d			String 137.5 v.d			Pipe Av. 150 v.d.		
O.	E.	m.v.	C.E.	E.	m.v.	C.E.	E.	m.v.	C.E.
P.	2.5	.6	+2.5	1.8	.5	—1.9	.9	.4	— .1
S.	1.0	.9	— .8	1.9	.8	—1.9	1.1	.5	— .3
Ma.	1.2	1.3	+1.2	1.2	.5	+1.2	1.3	.8	+ .6
V.Z.	1.3	.5	+1.2	2.1	.4	—1.9	.6	.5	— .5
Mi.	2.0	.6	+2.0	.6	.5	+ .3	.5	.4	— .2
Av. E.	1.6			1.5			.9		
Av. m.v.		.8			.5			.5	
Av. C.E.			1.5			1.4			.3
G.C.E.			+1.2			— .8			— .1

The results of this series of experiments are summarized in Table V. Judging by the magnitude of the average error and the constant error, the record is in favor of the organ pipe. This is probably due to the fact that this tone is most nearly like that of the human voice in tone-color, or timbre. The introspections of our observers, all of whom have good musical ability and were practiced in observation, are however not in accord with this. Four of the five stated that the string was the easiest standard to imitate. P, the one professional musician in the group, felt that he did best on the fork. But reference to the table shows that it was here that he made his largest errors and even the largest made by any observer on that standard. S. stated that the string was by far the best as a standard but made his smallest errors, and the smallest made by anyone, on the fork. It must be noted also that S. has had more practice with forks than any other member of the group. Practice

is undoubtedly a factor and the value of it for a particular observer depends chiefly on what associations are awakened by a given tone-color. Purity, for example, may be thought of as thinness, and secondarily as highness of tone. While tuning forks, being relatively pure and free from over tones, are at a disadvantage on the side of richness, it is also true that in most groups the observers are about equally unpracticed in singing with forks, which is an advantage from the standpoint of measurement. The forks also are decidedly more constant in pitch than any other type of standard tone. Two of the observers noticed a tendency to imitate the timber of the standards.

From the above observations it seems fair to conclude that richness of tone favors accuracy in the reproduction of any particular standard.[11]

EXPERIMENTS SERIES V: VOWEL QUALITY AND ACCURACY

Berlage (2) introduced the problem of the influence of vowel quality (or change in the timbre of the singing voice) upon accuracy in imitating pitch, and made measurements on this point for the purpose of determining the effect of mouth resonance upon the pitch of the reproductions. In considering the problem here there is no thought of discrediting the results found by Berlage. The tonoscope method of recording has enabled us to take many more records than were used by him in computing his results and the matter is of such far reaching importance that it seemed worth while to include in our study a series on this factor, limiting our measurements to the following vowels:

u as oo in "toot"
o as o in "no"
a as in "ah"
e as e in "there"
i as i in "machine"

In addition to these, humming the tone was introduced in the test as the "hum" seemed to have no marked vowel quality.

[11] Starch (23 p. 52) in his conclusions on the effect of timbre in the localization of sound makes this statement: "The richer and more complex a sound the more accurately it can be localized."

TABLE VI. Vowel quality of the voice and accuracy

O.	u	o	a	e	i	"h"	u	o	a	e	i	"h"	u	o	a	e	i	"h"
	E. m.v. C.E.		144 v.d.				E. m.v. C.E.		182 v.d.				E. m.v. C.E.		240 v.d.			
P.	.6	.6	.4	.8	.6	.5	.7	1.6	1.4	1.5	.5	.4	2.6	1.9	2.0	1.2	4.0	2.4
	.4	.6	.4	.5	.7	.6	.8	1.0	.8	.8	.5	.4	1.2	1.5	1.0	.7	.4	.8
	— .2	+ .1	— .2	0	+ .4	— .3	— .5	—1.4	—1.4	—1.5	0	— .4	+2.6	+1.4	+2.0	+1.2	+4.0	+2.4
S.	1.0	.8	.8	1.1	.9	.9	.9	.9	1.0	.9	1.3	1.0	1.4	1.4	1.4	1.5	1.4	1.3
	.7	.8	.5	.8	.6	.8	.7	.9	.7	.8	.8	.8	1.2	1.0	1.2	1.2	1.0	1.1
	— .9	— .1	+ .6	+ .8	+ .4	+ .7	— .9	— .8	— .7	— .6	—1.2	—1.3	— .7	— .6	—1.1	— .1	+ .7	+ .6
C.	.7	.9	.9	.8	2.2	1.3	.7	.9	1.1	1.0	1.0	.7	2.5	2.3	2.5	2.6	3.3	2.4
	.6	.4	.7	.8	.5	.6	.7	.7	.9	.9	.6	.7	1.2	1.3	.9	1.1	1.4	1.2
	+ .4	+ .6	+ .5	+ .3	+2.2	+1.1	— .2	— .8	— .6	— .4	+ .9	+ .1	+2.2	+2.1	+2.4	+2.4	+3.0	+2.2
V.Z.	.9	1.0	.5	.4	1.5	.4	.8	1.1	.6	.7	1.5	1.0	2.3	3.3	2.5	2.7	4.0	3.1
	1.1	.7	.5	.3	.4	.4	.4	.9	.5	.6	1.1	.8	1.1	1.7	1.1	1.3	1.6	1.0
	+ .9	— .4	+ .2	+ .3	+1.5	+ .2	— .5	—1.0	— .4	+ .1	+ .8	— .3	+2.0	+2.8	+2.1	+2.4	+4.0	+3.1
Wi.	2.4	1.6	1.9	2.1	4.7	4.2	1.4	1.1	1.3	1.2	1.0	1.0	2.1	1.4	1.1	.9	1.6	.7
	1.0	1.2	.8	.9	.7	.8	1.1	1.0	.9	.6	1.0	.9	1.5	1.4	.9	.8	1.3	.8
	+2.1	+1.5	+1.9	+2.1	+4.7	+4.2	+1.1	— .5	—1.2	+1.2	+ .2	+ .4	+1.4	+ .2	— .6	— .4	+1.4	— .2
Mi.	2.3	1.6	1.3	1.7	1.7	1.8	1.2	1.3	1.2	1.1	1.5	1.1	3.2	2.2	4.5	3.8	4.8	3.7
	1.7	1.5	1.2	1.7	1.6	1.9	1.2	1.3	1.3	1.1	1.5	1.1	2.1	1.5	2.1	1.9	1.8	2.0
	—1.8	— .9	+ .4	+ .3	+ .6	— .7	— .7	— .8	— .3	— .3	+ .5	— .4	+2.6	+2.2	+4.5	+3.8	+4.8	+3.7
Av. E.	1.3	1.1	1.0	1.1	1.9	1.5	1.0	1.1	1.1	1.1	1.1	.9	2.4	2.1	2.3	2.1	3.2	2.3
Av. m.v.	.9	.9	.7	.8	.8	.9	.8	1.0	.8	.8	.9	.8	1.4	1.2	1.2	1.2	1.2	1.2
Av. C.E.	1.0	.6	.6	.6	1.6	.9	.6	.9	.8	.7	.6	.5	1.9	1.5	2.1	1.7	3.0	2.0
G.C.E.	+ .1	+ .1	+ .6	+ .6	+1.6	+ .9	— 3	— .9	— .8	— .7	+ .2	— .3	+1.7	+1.0	+1.5	+1.5	+3.0	+2.0

Three forks of the large disc variety were used as standards, the pitches being: 144, 182 and 240 v.d. and each of these three tones was reproduced to the five vowels and the "hum" twenty times, a total of 360 trials for the individual observer. The test was divided between two equal periods. The order of reproducing was two trials on each fork to each vowel in the double fatigue order, illustrated as follows: 144 to *u*, 182 to *u*, 240 to *u*, pause, 240 to *u*, 182 to *u*, and 144 to *u*; then 240 to *o*, 182 to *o*, etc., followed by 144 to *a*, 182 to *a*, and so on throughout the test, the order of the vowels being *u*, *o*, *a*, *e*, *i*, and "hum." All standards were presented to the ear for a duration of approximately 2 seconds and an interval of 1 second was allowed before the singing.

That the vowel quality is a factor influencing the accuracy of reproduction is borne out by the results of the series as shown in Table VI. The average error (E) and the mean variation (m.v.) are given merely for an index to the reliability of the record. They are both large as compared with the constant error (C.E.) which is the factor in terms of which we desire to measure the effect of vowel quality on reproduction.

Although there are characteristic differences for the three pitch levels and for the different individual observers, the results in Table VI may be fairly represented by a single curve (Fig. 4). This

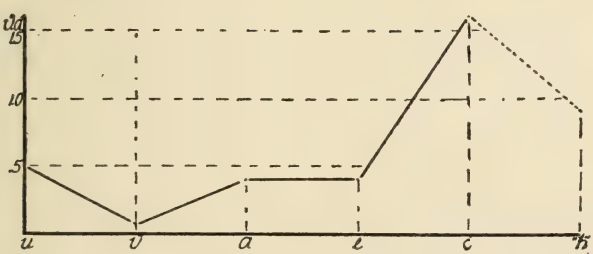

Fig. 4. Vowel quality of the voice and accuracy. (Table VI.)

shows graphically the algebraic average of the records (G. C. E.) for each of the vowels and for the three levels, 144 v.d., 182 v.d. and 240 v.d. There is a tendency for the vowels to fall into three groups: namely, (1) *o* sung the lowest, (2) *a*, *e* and possibly *u* sung moderately sharp and (3) *i* sung decidedly sharp. These facts would seem to point to the general conclusion that the higher the dominating overtone in a vowel clang, the higher that vowel will be sung. In Fig. 4, *u* offers the single exception to that rule.

The hum was supposed to be neutral as it was moderately weak and the record was made from the nasal breath. This assumption is confirmed by the record which gives the hum a middle place with *a* and *e*.

It must be remembered that there is, in the foregoing records which were sung on *a*, a tendency to sharp by about the amount of sharp for *a* here. That tendency is probably due to some other cause than timbre. It may therefore be suggested that *a* and *e*, the vowels usually sung when one is free, are fairly neutral; *o* (and possibly *u*) are sung relatively flat and *i* relatively sharp. This view, it will be observed, is confirmed by the hum.

Our results seem to differ radically from those of Berlage (4), second part of item 4, in the observations which are common to both. But our method also was radically different; moreover, his conclusion (item 4) is somewhat modified when we read in his article p. 76 where the results of the vowel experiments are discussed. "Accordingly one may look upon a slight increase in the variable error as probable with vowel change" (*i.e.* when the observer tries to reproduce his own pitch but on a different vowel) . . . "other generalities cannot be deduced for the table . . . "

These results in reference to vowel quality are of so far reaching significance for speech and song that we may not venture further discussion for the matter must be made a special object of investigation for verification of the empirical data and in search of an interpretation. It seems safe however to proceed in our work using *"a"* as the vowel quality for reproductions.

EXPERIMENTS SERIES VI: ACCURACY IN SINGING

Having gained some insight concerning the influence of voice range, standard tone intensity, voice volume, standard tone timbre, and voice timbre, on the accuracy of voice control, we now turn to the main problems of our research. These may be restated as follows: (1) What is the average error of the human voice in reproducing the pitch of a tone? (2) What is the average minimal producible change of the voice? (3) Is there any general tendency to sing sharp or flat? (4) How does the average performance of men and women compare on the above three points? All the studies referred to in the historical account contain results which cast light on some of these problems. But in almost every case these

results and problems are secondary to the main interest of the study; and moreover the number of observers and observations is usually quite limited. In Series VI therefore, we have made these problems the central issue on a large group of persons to give our results significance as norms.

Apparatus and method

Standards. With the aid of the tonoscope, eleven large disc forks were tuned to the following pitches: 128, 128.5, 129, 130, 131, 133, 136, 140, 145, 151, and 158 v.d. The series of pitch increments between the forks was therefore: .5, 1, 2, 3, 5, 8, 12, 17, 23, and 30 v.d. as measured from 128 v.d. This series of tones was used for men. For the women a second set was provided on 256 v.d. as a basis, namely, 256, 256.5, 257, 258, 259, 261, 264, 268, 273, 279, and 286 v.d. In this second set it will be noted that the same pitch increment (absolute) were used as in the 128 v.d. set instead of the relatively equal increments. In this respect the procedure was based upon the conclusions reached in Series I.

Koenig resonators were provided for each set of forks. As the increments were small it was found that one resonator would speak sufficiently well to several tones. In the case of the 128 set three resonators were used: first, 128 v.d. to and including 136; second, 140 and 145 v.d.; and third, 151 and 158 v.d. For the higher set two resonators were found sufficient: first, 256 v.d. to and including 268 v.d.; and second, 273, 279, and 286 v.d. Both series of forks as reinforced by the resonators gave tones of pleasing quality and medium intensity.

Observers. Two hundred and one individuals, ninety-four men and one hundred and seven women, took the test which is about to be described. This number comprised those enrolled in the elementary psychology courses in the University of Iowa, 1912-1913. Of these about one hundred fifteen were sophomores; the remaining were upperclassmen. None of them had had any practice in this test. Among them were some excellent vocalists and some others who claimed never even to hum or whistle and to have difficulty in recognizing old and familiar tunes if unaccompanied by words. No one was excused because of his inability and no one was selected because of ability, for it was desired in so far as possible to secure what might be considered an average group. A previous lecture on

the measurement of musical capacity had successfully aroused the interest of the observers so that they entered into the experiment with zest, many of them desiring to secure their individual results.

The charge. The instructions were given by word of mouth to each person, although the appointments were so arranged that one observer was present while another was taking the test and so became familiar with the procedure before he actually entered upon it. Supposing the observer to be a man the instructions would be as follows:

"Mr. ———, we have here a series of eleven tuning forks. This one (striking the 128 v.d. and presenting it before the resonator) is c below "middle c", it is a tone of 128 v.d., the lowest tone in the series; we will call it "o". This one (striking and presenting the increment fork 30, 158 v.d.) is considerably higher than o as you easily notice, and is the highest one in the group. These other forks all represent pitches between the two which we have sounded. The test to-day consists in singing these eleven tones one after the other as they are given. They will be presented in pairs. First we will sound the o, the lowest one of the tones; you will listen carefully to it and then sing a tone of the same pitch. Immediately after your singing, the highest tone in the group (30, 158 v.d.) will be sounded; you will listen and sing that one. Then the o will be sounded again and, after you sing it, there will come the next to the highest tone (23, 151 v.d.); and so on we will come down one step at a time always reproducing the o before each of the interval forks. When you have tried all the tones in the series you will go back over them in the reverse order. Simply imitate as nearly as possible the pitch of each tone as it is given, always remembering that the o is the lowest one in the series. Sing all the tones with a natural voice volume and use the vowel *"a"* (a as in "ah") and whenever you feel dissatisfied with any trial ask for a repetition."

Following these instructions, in order to put the observer at ease and to satisfy his curiosity, the experimenter gave a brief explanation and demonstration of reading on the tonoscope.

The test. The forks were presented to the resonator by a helper who gave his attention solely to the task of sounding the tones in the right order and with as nearly uniform intensity and duration as possible. The tones were sounded with medium intensity varying towards the "weak." The observer sat on a high stool or stood at

the side of the instrument in a position which kept him from seeing his own record. He sang the tones into a metal speaking-tube placing the lips lightly against the fingers of the hand which grasped the mouthpiece. The arm was supported by an adjustable rest and, so far as could be, strain and unnaturalness were avoided.

A few preliminary trials were given on increment 0-30 in order that the observer might find himself becoming somewhat familiar, not only with the tonal range covered by the standards but with the experience of taking pitch from a tuning fork. The series was then given in pairs in the following order: 0-30, 0-23, 0-17, 0-12, 0-8, 0-5, 0-2, 0-1, 0-.5; 0-.5, 0-1, 0-2, etc. back to 0-30. The complete test consisted in singing the series thus five times. This gave one hundred reproductions of the 0, and ten on each of the increment tones, a total of two hundred tones for each observer. This series therefore contains forty thousand records. The test as outlined could not be performed with care in less than 30 minutes. In some cases and especially with non-musical persons a much longer time was required than this.

Throughout the test we endeavored to keep the observer seriously trying to sing the exact pitch of the forks. To this end it was deemed desirable to offer some encouragement, especially during the first fifth of the experiment, no matter how poor or good the record. It was observed that encouragement did not cause the singers to be self-satisfied or careless but rather served to make them try the harder. It helped moreover to create an atmosphere of ease and naturalness. But while there was encouragement there was also some criticism. If, for example, the observer was singing the 0 flat 5 or 6 v.d. regularly he was told to listen more carefully to the standard and to make sure that he had the right pitch but no intimation was given as to the character of the error. Little rest periods of twenty seconds were rather frequent and were found to be of much service. Many times it was noted that after such a period the errors were decidedly smaller than before.

A few questions concerning the observer's musical education, voice range, and ability to play and sing were asked during or following the experiment and the answers together with some comments regarding his performance of the test were made matters of record.

Justification of procedure

Before considering the results of this series it remains to justify the form of procedure as outlined above in the light of the sources of error revealed by our previous experiments and by those of other investigators.

Voice level of test. Our experiments (Series I) on the accuracy of pitch singing within the voice range demonstrated that the errors are relatively smaller on the higher tones. Unpracticed observers however, will much more readily try a tone that is medium or low than one that is high. It therefore seemed best for general testing to choose a voice level which all would recognize as being well within range. The selection of 128 to 158 v.d. for men and of 256 to 286 v.d. for women is thus the result of considerable experience in testing groups of individuals, and seems further justifiable on the grounds of pitch discrimination as previously stated.

Forks for standards. Tuning forks were retained for standards even though the records of Series IV indicate that the organ pipe and dichord can be imitated more accurately. Forks are very simple, easily manipulated, of practically constant timbre, and at the same time reliable in pitch. And if, as in our test, a series of tones differing from each other by slight degrees of pitch is desired to be sounded in rapid succession, tuning forks are the most reliable apparatus. Furthermore they are used so little for general musical purposes that in testing with them no group of observers is given undue advantage.

Many standards *vs.* one standard. Berlage (2) found that his observers could reproduce their own voice tones more accurately than tones given by some one else, the increase of precision showing itself chiefly in a decrease of the constant error. We have frequently noticed a tendency, which is a corrolary of Berlage's conclusion. Observers when making successive trials on the same standard very often reproduce their own reproductions rather than make new efforts at imitating the real standard. The observer finds it much easier to reproduce his own previous tone, duplicating the muscle tension and mouth resonance which he experienced at that time and felt to be satisfactory. Indeed, even though he conscientiously work against this tendency, he can not overcome it entirely if engaged in making successive trials where the pauses between are brief. This is confirmed by the fact that frequently when observers

for some cause or other have been dissatisfied with attempts and desired new trials giving them immediately they would in the new trials unconsciously repeat the identical pitch given before. This same tendency has sometimes been evident even in large and unusual errors which the experimenter might rule out, asking for new trials. In view of these considerations it seemed best in our general test to adopt the principle of many standard tones and no successive trials on the same tone.

The increments between the forks were made small and of varying magnitudes for two reasons: first, in using these small increments we do not complicate our work with the factor of musical intervals, and second, in using a series of small increments we make possible the measurement also of the ability to make faint shadings (sharp or flat) in the pitch of the voice. The selection of increments is arbitrary. These particular steps were chosen because they have been found satisfactory in work with pitch discrimination (21) at the level of 435 v.d. and, as stated before, extensive research by Vance (26) and others shows that pitch discrimination is practically constant in terms of vibration frequency in the middle range of tonal hearing here covered. This is also the ground for making the increments for the women the same number of vibrations instead of the relative parts of a tone, in which case they would have been doubled.

Sounding the two tones. Seashore and Jenner (19) employed the method of "least producible, or minimal, change". The observer sang the standard or a tone at a given interval from it and then reproduced his own reproduction, save that he made it "the least possible" sharp or flat according as the experimenter might direct. While this will undoubtedly become a standard method in extensive work with an observer it is not suited to tests of a single sitting, first, because ability is rapidly improved by practice and, second, because the observer tends to be easily satisfied with his effort. The better way is not to rely on the changing subjective standard of the observer but to provide a series of constant objective increments and give him the opportunity to find his own level as by the method of constant stimuli in lifted weights or pitch discrimination. Such a series has been provided in the standards and increments mentioned above.

Order of standards. Manifestly the standards might be presented

to the observer in any one of a number of different orders. After trying out the matter thoroughly with the help of three good observers we selected the order of presentation above described for the following reasons: (1) to give the tones in pairs (0-30, 0-23, etc.) takes direct advantage of all the latitude which the series provides. Most observers can easily detect the difference 0-30, while many (theoretically about 25 per cent.) would be baffled to find a difference between 23 and 30; (2) to begin with the largest increment and work towards the smallest has the double advantage of establishing confidence in the attitude of the subject and of stimulating effort; (3) to give the increments in a series and in double fatigue order rests the voice from the unusual strain of making the least producible change, and (4) to explain definitely at the beginning of the test that all the increments are in one direction, *i.e.* above the 0, simplifies the problem and puts it more definitely under control than if uncertainty as to change of direction in standards were allowed. The test is therefore not to measure the judgment for direction of pitch difference but the judgment and expression of the amount of pitch difference between two tones. In pitch discrimination it is well known that much depends upon the direction of the expectant attention.[12] And should we present the standards of our test in a chance order we would complicate it exceedingly at the critical point of the smallest increments.

Time intervals. At the very beginning of the test the intention was to allow an interval of 1 second between the breaking off of the standard tone and the singing by the observer. But the method

[12] An idea of the influence of this same source of error operating in the field of singing may be gained from the following illustration. The author in instructing a very fine observer thoughtlessly said, (the error was altogether unintentional); "We have here two forks, the first, 128 v.d., and the other one 3 v.d. higher, 131 v.d. You will please sing them one after the other. I will give the lower one first." Then the forks were presented and reproduced as directed. When we came to the twelfth trial the observer remarked: "I seem to feel strain to bring the 131 v.d. up". In the moment of reflection following this remark the writer recognized that he had made a mistake in instructing the subject, as the so-called "131 v.d." was really 3 v.d. lower than 128 v.d., or 125 v.d. We find in the twelve trials made that the average reproduction of 128 v.d. is 123.6 v.d. while the average pitch given for the supposed 131 v.d. (really 125 v.d.) is 124 v.d. The misunderstanding and therefore expectant attention changed the direction of the reproductions, and brought in much larger constant errors than are usual for this individual. It should also be noted that the errors are minus.

was soon given up as, in this case, cumbersome and unpractical, and furthermore we did not care to complicate our test with the factor of tone memory. (See Berlage (2) and Sokolowsky (22)). The observers in their usual singing with musical instruments make no such perceptible time intervals. They sing with the tones of the instrument, perhaps holding them somewhat longer than is done by the instrument. When the standard has been sounded the attention is centered, the muscles of the larynx almost involuntarily assume a particular tension and it is unnatural to wait for the beating of a metronome or some other signal to begin singing. If the unpracticed observer is told to make his own interval, unless checked up diligently, he will very soon be making intervals that are exceedingly short, if indeed he is not singing simultaneously with the standard tones. The method followed therefore was to sound the forks for approximately 1 second, encouraging the observer to begin his tone during the sounding of the fork and to hold it longer than the fork.

It may be objected that one might sing fairly accurately judging simply on the secondary criterion of beats between his voice and the standard tone. Helmholtz indeed (9 p. 326) suggests this as a convenient method for the singer to use for checking his own accuracy in practice exercises. While it would be possible for a highly practiced observer it can hardly have much influence in our test. The author made it a point to question frequently regarding the way observers judged of their success in reproducing tones and was not able to find any one who knowingly made use of this criterion. It is however quite possible that the roughness of 6 or 8 or more beats per second may occasionally have caused some observer to be dissatisfied with his attempt. But the tonal fluctuations and adjustments which are necessary to bring about a lessening of the frequency of beats between the voice and an outside standard are easily recognized with the tonoscope; no such "finding" process was observed.

Another time interval which must be considered is that between the o (128 or 256 v.d.) and the increment fork of any particular pair. In order that the standards for minimal change of voice may have their greatest value the interval indicated must be as short as possible admitting of a quick, direct comparison of tones; otherwise the test practically resolves itself into the singing of a single

tone. Hence the presentation of the increment fork followed immediately upon the close of the observer's reproduction of o, the subject being encouraged to make the reproduction about 1 second in length. The increment forks were struck while the o was being reproduced. This was, however, no distraction as only a slight blow on the practically noiseless sounder was necessary, and the forks could not be heard until presented before the resonators. Following the reproduction of each increment fork there was a period of about 2.5 seconds before the next sounding of o.

Other factors. In the matter of intensity of standard and intensity and vowel quality of the voice we took direct advantage of our previous work and adopted such conditions as would give the most normal results according to those findings. By the use of resonators at a considerable distance from the observer's ear we found a satisfactory means of controlling the intensity of the standards,[13] while the intensity of the voice had to be judged subjectively and watched by the experimenter. And in the selection of *"ä"* we are using that vowel quality which according to Berlage and our own results affects least the constant error of the reproductions.

Tables of data

The constant error (C. E.) and mean variation (m.v.) were found for the ten trials on each fork of the ten pairs given in the test. These twenty C. E.'s and twenty m.v.'s for each individual tested are embodied in Table VII, which has been divided into two parts, A. and B., for the men and women respectively. In the first column of the table, at the left, are given the numbers which stand for the individual observers. This numbering is in no sense a ranking, but simply for convenience in handling the data and aid in identification. Odd numbers are used throughout to refer to women and even numbers to men. The second column from the left shows the C. E. and m.v. (the latter is under the former) for the ten trials on o. when used in the pair o-30. The same measures for the ten trials on variant (or interval) tone 30 are given in column three, and each of the successive smaller increments are represented in the same manner. The arithmetic averages for the constant error

[13] It is possible that the pitch of a standard is not only varied by its intensity but also by its position when held near the ear.

TABLE VII, A. Accuracy in singing: men

Obs.	0-30		0-23		0-17		0-12		0-8		0-5		0-3		0-2		0-1		0-.5		Average Arithmetic O	Average Arithmetic V	Average Algebraic O	Average Algebraic V
	128	158	128	151	128	145	128	140	128	136	128	133	128	131	128	130	128	129	128	128.5	128	138	128	138
2	−0.2	+4.0	−0.3	+2.7	+1.0	+4.6	−0.1	+8.9	+0.1	+7.6	−0.2	+11.4	−0.1	+9.5	+0.5	+8.2	+0.8	+5.7	+1.3	+4.3	.46	6.69	+.29	+6.69
	1.8	2.2	2.1	2.5	1.0	1.8	1.5	2.3	1.7	4.0	1.6	2.8	2.1	2.1	1.9	1.8	2.2	2.5	1.5	1.4	1.74	2.34		
4	−1.9	−6.6	−2.9	−2.3	−1.3	−0.3	−3.0	−1.5	−4.8	+4.6	−2.4	+0.0	−3.5	+7.7	−3.8	+5.9	−2.3	+10.5	−3.3	+5.5	2.92	5.09	−2.92	+2.90
	2.7	4.8	1.9	5.5	1.5	7.7	2.8	6.9	1.4	6.8	3.0	6.8	1.7	5.7	1.6	6.3	1.5	8.3	2.1	4.8	2.02	6.30		
6	−0.1	+1.7	0	+4.5	−0.2	+4.7	+0.1	+5.0	0	+6.0	+0.1	+4.5	−0.2	+4.2	−0.2	+3.4	−0.3	+3.5	+0.1	+3.0	.12	4.05	−.06	+4.05
	1.1	.7	.8	1.9	1.0	3.1	1.1	4.0	.4	3.4	.9	2.5	.6	1.2	.6	1.4	.8	1.3	.5	1.4	.78	2.00		
8	+1.0	+3.6	+1.1	+2.9	+1.3	+5.2	+1.2	+5.5	+0.9	+3.9	+0.8	+4.7	+1.1	+5.4	+0.9	+5.6	+0.7	+5.1	+0.9	+5.1	.99	4.70	+.99	+4.70
	.4	1.2	.7	1.3	.9	3.	.4	1.9	.3	1.7	.4	1.7	.5	2.6	.7	1.8	.5	1.7	.3	1.4	.51	1.83		
10	+0.3	−0.1	+0.3	+0.7	+0.1	−0.1	0.0	+2.8	0.0	+4.5	+0.1	+4.8	−0.2	+3.6	−0.2	+3.9	+0.1	+2.6	0.0	+1.7	.13	2.58	+.05	+2.54
	.7	.5	.7	1.1	.5	2.3	.4	1.0	.4	.9	.1	1.2	.6	1.4	.8	1.5	.5	1.2	.2	1.2	.49	1.23		
12	−0.1	+1.2	+0.4	+0.8	+0.3	+3.0	+0.3	+2.3	+0.4	+2.3	+0.2	+3.0	−0.2	+3.3	+0.1	+3.2	+0.1	+3.2	+0.3	+2.3	.24	2.46	+.18	+2.46
	.7	3.0	.6	2.2	.7	3.8	.7	2.7	.6	1.9	.8	1.2	.2	1.1	.5	1.4	.7	1.8	.5	1.0	.60	2.02		
14	+4.0	+17.3	+2.0	+21.8	+1.3	+25.0	+4.0	+26.5	+3.5	+28.8	+0.3	+36.0	−0.2	+34.5	+0.5	+27.3	+1.1	+33.0	+0.5	+28.0	1.74	27.0	+1.7	+27.8
	[illegible]																							

TABLE VII, B. Accuracy in singing: women

Obs.	0-30		0-23		0-17		0-12		0-8		0-5		0-3		0-2		0-1		0-.5		Average Arithmetic O	Average Arithmetic V	Average Algebraic O	Average Algebraic V
	[illegible]																							

TABLE VIII. Summary from Table VII

A. Men

Ave.	0-30		0-23		0-17		0-12		0-8		0-5		0-3		0-2		0-1		0-.5		Average Arithmetic O	Average Arithmetic V	Average Algebraic O	Average Algebraic V
	128	158	128	151	128	145	128	140	128	136	128	133	128	131	128	130	128	129	128	128.5	128	138	128	138
M.V. (v.d.)	1.49	2.38	1.53	3.01	1.47	3.26	1.01	3.43	1.53	3.28	1.54	3.21	1.61	3.25	1.53	3.10	1.62	2.89	1.54	2.72	1.54	3.05		
% + C.E.	33	68	32	83	33	88	32	96	31	100	32	99	31	100	34	96	35	93	34	89	33	91		
% − C.E.	66	32	61	16	60	9	65	3	64	0	67	0	66	0	63	4	64	5	64	11	65	8		
% o C.E.	1	0	7	1	1	3	3	1	5	0	1	1	3	0	3	0	1	2	2	0	2	1		
C.E. (v.d.)	1.30	2.59	1.53	3.28	1.47	4.18	1.55	5.34	1.59	6.81	1.47	7.15	1.55	6.97	1.62	6.35	1.66	5.64	1.61	4.85	1.54	5.32		
G.C.E. (v.d.)	−.38	+1.00	−.47	+2.56	−.51	+3.98	−.53	+5.31	−.63	+6.81	−.53	+7.15	−.51	+6.97	−.57	+6.27	−.57	+5.55	−.64	+4.71			−.53	+5.03

B. Women

Ave.	0-30		0-23		0-17		0-12		0-8		0-5		0-3		0-2		0-1		0-.5		Average Arithmetic O	Average Arithmetic V	Average Algebraic O	Average Algebraic V
	256	286	256	279	256	273	256	268	256	264	256	261	256	259	256	258	256	257	256	256.5	256	266	256	266
M.V. (v.d.)	1.23	1.63	1.27	2.10	1.25	2.30	1.25	2.32	1.28	2.30	1.35	2.50	1.28	2.45	1.27	2.29	1.38	2.00	1.39	1.92	1.29	2.21		
% + C.E.	76	73	74	90	75	97	66	98	65	98	65	98	63	98	63	97	66	97	68	94	67	94		
% − C.E.	23	24	23	10	22	3	31	1	34	2	30	2	32	2	36	3	37	3	30	5	30	5		
% o C.E.	1	3	3	0	3	0	3	1	1	0	5	0	5	0	1	0	3	0	2	1	3	1		
C.E. (v.d.)	1.56	1.69	1.57	3.13	1.63	4.23	1.54	5.48	1.52	6.30	1.59	6.71	1.73	6.26	1.81	5.74	1.79	4.98	1.77	4.06	1.65	4.86		
G.C.E. (v.d.)	+.99	+1.04	+.91	+2.80	+.92	+4.12	+.74	+5.46	+.71	+6.29	+.86	+6.56	+.70	+6.10	+.66	+5.50	+.14	+4.05	+.60	+3.85			+.77	+4.18

(C. E.) and the mean variations (m.v.) on both standard and variant are presented in the two columns headed O. V. (Arithmetic). The algebraic averages for the constant errors (C. E.) on both standard and variant are given in the two O. V. columns at the extreme right of the table.[14]

The consolidated footings for Table VII, section A and B, are given in Table VIII. The top notation is thus the same as in Table VII. A contains the final footings for men and B for women. The footings are set out as follows: Ave. m.v. is the average mean variation for the respective points in terms of vibrations; C. E. % +, the per cent. of individuals who made a constant error in the direction of a sharp; % —, the per cent. of those who flatted; % O, the per cent. of those who made no appreciable constant error in the ten trials; C. E., v.d., the average magnitude in vibrations of the constant errors, without regard to sign; and G. C. E. v.d. the tendency of the constant erors for the group, the algebraic mean. At the right, the grand averages for both groups are presented under the headings designated above.

Comparison of the abilities of men and women

The most striking general feature of these experiments is the the fact that women show the same ability as men, vibration for vibration, although the women sang an octave higher than the men.

The data on which this assertion is based may be traced most readily in the curves, Figs. 5-8, 10. In Fig. 5 C. it is seen that the curves for the average constant errors on the standard as well as on the variant practically coincide. On the standard they are almost straight lines, the variation for the men being from 1.36 v.d. to 1.66 v.d. with an average of 1.54 v.d., while in the case of the women the variation of this measure is from 1.52 v.d. to 1.81 v.d. with an average of 1.65 v.d. The curves for the variants do not come so near coinciding; they are of the same form, but the women have the advantage, their range of C. E. falling between 1.69 v.d. and 6.71 v.d. with an average of 4.86 v.d., while that for the men lies between 2.59 v.d. and 7.15 v.d. with an average of 5.32 v.d. As further confirmation of the fact that the average con-

[14] There would be little gained by placing E., the crude error, in our table as this measure is something of a cross between C. E. and m.v. and serves simply to indicate the distribution of the constant errors.

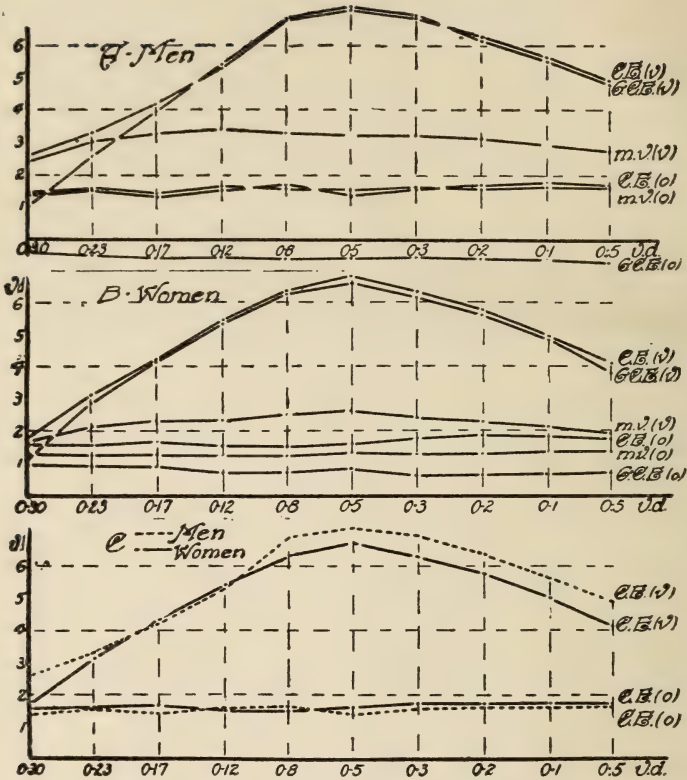

Fig. 5. The data in Table VIII. If there had been no errors all curves would coincide with the base line. The amount of deviation is indicated at the left in terms of vibrations: the increments on the base line. O denotes the standards (128 v.d. for men and 256 v.d. for women); V the variants; C. E. average (arithmetic) constant error; G. C. E. the algebraic constant error or general tendency of the group; and m.v. the mean variation. G. C. E. above the base indicates plus or sharp and below minus or flat.

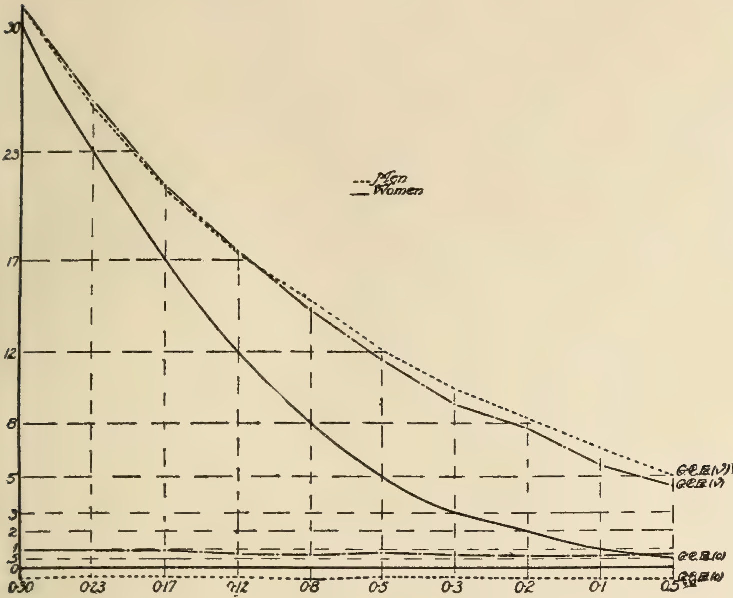

Fig. 6. Intervals as sung. (Table VIII). The distribution of the group constant errors (G. C. E.) for the standards (128 and 256 v.d.) and the variant in each interval. The intervals represented by the forks are shown in the heavy solid curves with which the other curves would coincide were there no errors in singing.

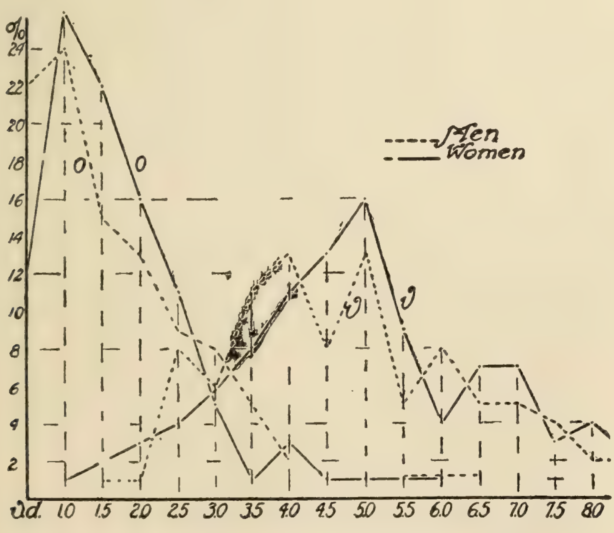

Fig. 7. The distribution of the average constant errors of all intervals for each observer with reference to the magnitude of the error. The data for this figure are found in the columns headed Arithmetic Average in Table VII. O, the standard tone: V, the variant.

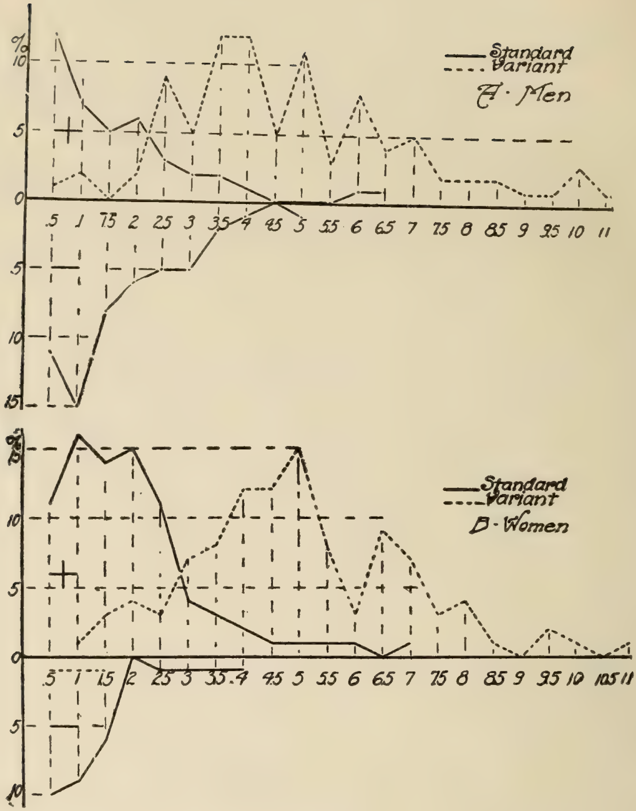

Fig. 8. Distribution of constant error, flat being denoted by — below the base and sharp by + above.

stant errors for both men and women represent approximately equal magnitudes attention is called to Fig. 7 in which is presented the distribution of the average constant errors of all intervals for each observer with reference to the magnitude of the error. The men have a slightly better record on the O. but the women have a more than compensating advantage on the V.

A corresponding agreement in the records for men and women is seen also in the constant tendency for the group (G. C. E. Fig. 5 A and B, 6, and 8 A and B). While the women tend to sharp and the men to flat on the standard (see Fig. 6) the amount is not far from equal in the two cases. (Cf. Table VIII, 65 per cent. of men flat on O while 67 per cent. of women sharp). In view of the general tendency of both men and women to sharp on the variant this difference in the tendency on the standard gives an advantage to the women as regards accuracy in the singing of the interval. An advantage which amounts to an average of over 2.0 v.d.

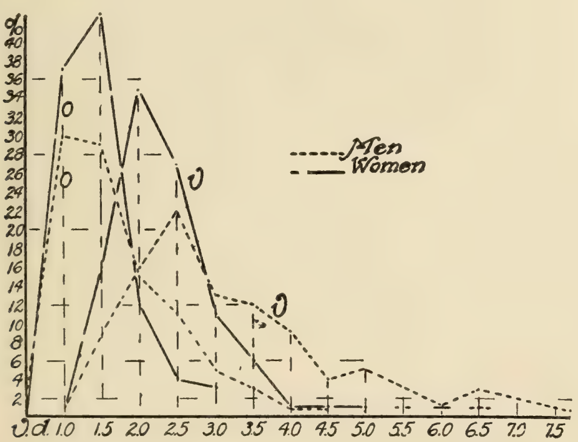

Fig. 9. The distribution of the mean variation (m.v.) for individuals (Table VII, average m.v. at right) with reference to the magnitude of the variations. O, m.v. of the standard tone; V, m.v. of the variant.

In the mean variation (Figs. 5 and 9), which is an important criterion, the advantage is more clearly in favor of the women, particularly in the singing of the variant. There are more men than women with a relatively large variation: but the mode in the case of O is slightly better for the men than for the women. The averages

of the men (Table VIII) are 1.54 and 3.05 v.d. as against 1.29 and 2.21 v.d. for women.

Taking all the data into account the general balance of all scores results practically in a draw:[15] men and women sing with equal accuracy (in terms of number of vibrations of error) although the former sing at 128 v.d. and the latter at 256 v.d. If on the other hand we count the error in relative parts of a tone instead of vibration for vibration, the women sing twice as accurately as the men. It may, however, be shown that the former statement represents the more logical point of view.

This result is in harmony with the results found in Series I with reference to accuracy within the tonal range. It was there found that so long as the singer was certainly within his natural range the man could sing the two tones here considered, 128 v.d. and 256 v.d., with nearly equal accuracy, in terms of vibrations and that, therefore, he tended to sing the higher twice as accurately as the lower. The difference here discussed is therefore not peculiarly a sex difference, but distinctly a matter of psycho-physic law of voice control within the tonal range. Men and women have equal ability in pitch discrimination (reference 21 p. 44), so also in voice control they have equal ability level for level within the tonal range. The fact however remains that women's voices are pitched in a higher register than men's voices and therefore, from the musical point of view, they can sing their tones relatively more accurately.

This result is, after all what we should expect for the principal limit upon accuracy in singing is accuracy in hearing and we know that both men and women can hear a difference of, *e.g.,* 1 v.d. as easily at 256 v.d. as at 128 v.d.

The mean variation

Fig. 5 shows that the mean variation is larger for the variants than for the standards. This is because the former are more difficult. It should be noted that this difference in the mean variation is a measure of the relative difficulty of the two tones as felt

[15] The following facts are significant: (1) there are fewer poor observers among the women; (2) women have smaller mean variations than men; and (3) women more nearly reproduce the intervals. It seems quite likely that in a mixed college group such as we have here, the women give more attention to vocal music than do the men, which may account for their superiority in this test.

and would also be a measure of the relative degree of accuracy in the singing of them were it not for the operation of the two motives for sharping the variant about the middle of the series of the increments. The fact that the mean variation is unaffected by the operation of these two motives is an indication of their fairly rigid operation.

The constant error

Figs. 5 and 6 show that the singing of the standard tone is not affected by the magnitude of the increment to be sung. The constant error is small and uniform. This is due partly to the fact that the standard tone was the same in all trials and therefore tended to become more or less automatic, and partly to the fact that the standard was sung first and that therefore the difficulty in marking off the interval would tend to crop out in the variant tone.

The singing of the variant follows the law that (1) all these small increments are overestimated and that (2) this overestimation increases gradually from the largest interval (0-30) and reaches a maximum in the cases of both men and women (Fig. 5, A and B) at the 5 v.d. interval from which it gradually again diminishes.

There are probably several motives operating to produce this overestimation; the fact that the maximum falls in the increment 5 v.d. points to a relationship between the hearing and the singing of the interval. The median for the least perceptible difference in pitch for this same group of individuals falls on 3 v.d. The increment 5 v.d. in singing would therefore represent one of the smallest increments actually heard. The distribution around this would be analogous to the distribution of the records in pitch discrimination for this group.

It is probable that, as in visual perception of space, all small angles are overestimated, there is in hearing of pitch a tendency to overestimate the smallest increments perceived. If we represent the uniformly increasing series of increments of pitch difference as a sharp wedge the apparent magnitude would be represented by a wedge blunted and thickened.

The operation of such a principle has been demonstrated for hearing in the matter of localization of sound. Starch (23) found that when a correction is made for the least perceptible change in the direction of the source this correction is always overdone.

The lack of fine control of the voice to reproduce the smallest

differences that are heard is another element involved. This factor is partly due to lack of knowledge and practice in this kind of voice control. The small differences which are actually heard larger than they really are, are sung still larger on account of this general lack of control for the making of fine shadings in pitch. This overdoing of a difference may perhaps be regarded as another phase of the same principle as the overestimation of small differences in pitch in hearing. At any rate the enlarging of the small discriminated increments is without doubt much increased in the singing. These small increments are overestimated in hearing (when heard) and are again overdone in the singing; and that this enlarging is proportionate up to the threshold for pitch discrimination.

In applying these principles to the interpretation of the relative magnitude of the errors in the singing of these increments we must bear in mind that where the small differences are not heard there would be a tendency to repeat the standard in trying to sing the variant—this happens not only because the difference is not heard, but even when an effort is made to sing an imperceptible sharp theoretically known to exist there is a tendency for the voice to "fall into the groove" of the standard tone which has been sung immediately before.

On the other hand it seems reasonable to take account of the fact that in this test we are asking the observer to do something with which he is almost entirely unfamiliar. In the larger intervals he recognizes differences but overestimates and oversings them. This overestimation increases regularly from the largest interval, 0-30, to 0-5, as was above noted. At 0-3 most of the observers fail to hear the difference because the conditions of the test do not provide the immediately successive presentation which is most favorable for the discrimination of pitch differences. Therefore, at 0-3 failing to hear the second fork higher, recognizing that he has not yet reached the smallest possible interval, and knowing that the second fork is higher than the O, our observer concentrates his attention, trying harder and harder until the last interval is sung. He is in large measure freed from the factor of overestimation in hearing for he hears no difference. He will very likely tell you that the forks sound just alike, but he knows and is reminded that the second one of each pair is higher. This knowledge forms the basis of his control of the voice. Quite naturally under the circum-

stances he resorts to the tendency (noted above) to take his cue for the second tone not from the fork but from his own previous tone. He "falls into the groove", however, just long enough to get his bearings, then sharps from this point, the magnitude of the sharp being governed roughly by the subject's pitch discrimination ability. In about 8 per cent. of the individual records of 0-.5 the records on the .5 v.d. are not sharp or may be slightly flat; in other words, the observers took the risk of making no sharp.

Applying these factors in the interpretation of the error in the singing of these small intervals of different magnitude, we find that, (1) the average overestimation is relatively small for the smallest increments because in many cases the difference is not heard and in singing a very small interval the voice uses its previous reproduction as the standard, sharping from it, and (2) the overestimation of the small increment is greatest for the smallest increments perceived and gradually diminishes as the increments grow larger so that it tends to disappear on the average when the magnitude of a half-tone is reached. Therefore, our test seems to have met the conditions for measuring the minimal producible change in the pitch of the voice. The increments from 0-30 to 0-5 serve to work down the voice, to make clear to the observer what is to be done, and to center his attention for most careful control. The four smaller increments, 0-3 to 0-.5 are the place where the "ability to make faint shadings" is really tested and under usual conditions the reproductions on the smallest increment, 0-.5, would seem to give the best measure.

If from the records on 0-.5 (algebraic C. E. or G. C. E.) we compute the magnitude of the smallest interval as *actually produced* by the individual observers and distribute these magnitudes according to their frequency, we have the curves of Fig. 12. The median value of the measures represented in Fig. 12 is 4.0 v.d. for women and 4.5 v.d. for men. There are more extremely poor observers among the men so that the average smallest intervals produced are 5.6 v.d. and 3.7 v.d. for men and women respectively.[16] These median values are in harmony with the results for pitch discrimination and may be taken as measures of the ability to produce minimal changes sharp or flat in the pitch of the voice.

[16] A part of this difference between men and women is to be accounted for in the fact that on the average the men flatted the O.

Dr. D. A. Anderson made a test on "minimal change in the pitch of the voice" in the Iowa Psychological Laboratory in 1909. His observers imitated the pitch of one standard fork and then sang the tone the least possible sharp or flat according as directed, making ten successive trials in each direction. There were 115 women and 65 men in the group tested. From the unpublished results of this test we learn that the average minimal producible change for men was 5.5 v.d. and for women 4.6 v.d as against 5.6 v.d. and 3.7 v.d. in our test. In comparing these results it must however be noted that 45 of Professor Anderson's poorest observers, most of them men, made no records which entered into his averages.

Seashore (19) reports the results of some tests of "minimal producible change" given to a small group of observers. The average records for six men on five successive days are as follows; 3.4, 3.5, 3.0, 2.6, and 2.7 v.d. Evidently the factor of practice entered here. However, the average of these results, which represents the only other available data on this ability in voice control, falls on the mode of our curve (Fig. 12) for men.

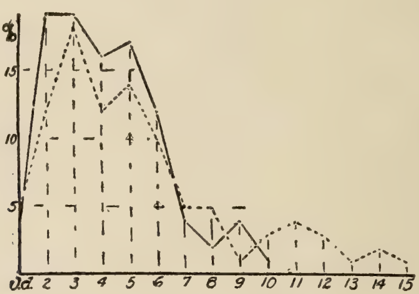

Fig. 10. Distribution of the magnitudes of the smallest interval actually produced by men and women. The method of computing the average magnitude of the smallest interval produced by each observer is illustrated in the following example: if the C. E. on O is —.9 v.d. and on V is +1.4 v.d. then the produced interval would equal the difference between —.9 v.d. and +1.4 v.d. plus .5 v.d. (the real step between the forks) = 2.8 v.d. If the C. E. on O is plus it is of course subtracted from the sum of C.E. on V and .5 v.d. Men dotted line; women broken line.

The average constant error (C. E.) on the standard is small and uniform, as is also the mean variation and the constant tendency for the group, (G. C. E. on the standard). Accuracy in the standard is not influenced by any difference in the magnitude of the in-

crements. This is chiefly because the standard tone was sung before the variant was sounded, and partly because a sort of "rut" was formed for the singing of the repeated standard.

The researches previously reviewed contain scattered measures on this ability. Klünder (1) found that he could reproduce an organ tone of 128 v.d. with an average crude error of .47 v.d. He rejected however the records of some other observers who showed larger errors. Cameron (4) worked with seven observers and tried a number of organ tones. The records by three of these observers gave an average error of about 6.6 v.d. Berlage (2), whose three observers reproduced voice tones, does not give the pitch of the standards. The average error for the three men, singing with an interval of from 1 second to 2 seconds, is .50 v.d.[17] Seashore (19) gives 1.2 v.d. as the average error of 100 trials by each of six men, on standard 100 v.d. Sokolowsky (22) with his seven professional singers finds an average error of 1 v.d. at the average pitch of 251 v.d.

The group constant error

Throughout the previous pages there have been references to the tendency of both men and women to sing sharp when reproducing a tone. The difference in the direction of this error in the standard for men and for women is so constant that, while small, it points to some motive in the character of the tone, the mode of singing, or some tendency characteristic of a given pitch level. The distribution seen in Figs. 7 and 8 shows that the sharps and the flats are not far from equal both in the number and the magnitude for men; for women the sharps predominate in both magnitude and number.

Cameron (4) noticed this tendency and called attention to it. In his experiments it appeared especially in sustained tones.[18] We have not worked with sustained tones but have found the same

[17] Berlage's tables are needlessly complicated by his using the signs with opposite from the usual meaning.

[18] Berlage (2) did not find this tendency to sharp and was surprised, but we must remember that he worked with voice tones for standards (the richest tone possible) and our experiments seem to show that with rich standard tones the sharping of the constant error is considerably decreased. Sokolowsky's (22) results are also negative as regards any general tendency for both sexes to sing sharp. The errors on the twenty tones sung by women, however, *show an algebraic average of +1.03 v.d.,* although eleven of these tones were sung flat.

tendency with reproductions of one and two seconds in length. Reference to our tables (G. C. E.) will show that almost without exception sharping is the predominant direction of the constant errors in all six series of our experiments. The tendency to sing sharp is not materially affected by the level of the pitch so long as the tone remains within the range of the voice; it is increased by loud volume of voice, weak volume of standard, certain vowel formants such as are found in "e" and "i", and by purity of the standard tones.

The best cases

The question naturally arises, to what extent the presence of a few cases of very large error affect the averages. To cast some light on this and also to gain an idea of the performance of the best observers in the group the author made a selection of twenty-five persons of each sex. The selection was made chiefly on the basis of a small Ave. m.v. in the standard (0). The size of the Ave. C. E. of 0 and the Ave. m.v. for the increments, were used as secondary criteria. There are some records, for example N. 9, which from the standpoint of the constant errors alone are very near the ideal curves, but because of rather large mean variations must be omitted from these selected groups. The selection of women was as follows: Nos. 1, 3, 13, 15, 21, 55, 61, 63, 77, 85, 93, 97, 105, 107, 113, 117, 125, 153, 159, 169, 177, 181, 183, 201, and 209. The men's records chosen were: Nos: 6, 8, 10, 12, 16, 28, 50, 62, 68, 72, 82, 88, 102, 106, 110, 114, 120, 126, 128, 144, 146, 148, 154, 156, and 164.

The separate tabulation of these fifty supposedly best cases reveals the presence of the same general tendencies in these selected groups as have been noted in the large groups, with the difference that they are not so pronounced and that here the men in a relative comparison make a better showing than the women, in that their overestimation especially of the smaller pitch increments is less. Therefore blame for the large errors (overestimation of intervals) can hardly be shifted to a few individuals as indeed we might have shown by referring to Figs. 7 and 8 which demonstrate that the distribution of the errors forms fairly normal frequency curves.

Correlation of singing with pitch discrimination

Pitch discrimination records are available for eighty-two of the men, and one hundred and four of the women who acted as observers

in our tests. The well-known formula of the Pearson "Product-Moments" was employed and resulted in the following correlations:

		r	P.E.r
Men:	Size of ave. smallest interval produced with Pitch. Disc.	+.21	.072
	" " " m.v. on O. " " "	+.04	.074
	" " " C.E. on O. " " "	+.08	.074
	" " " m.v. on V. " " "	+.33	.066
	" " " C.E. on V. " " "	+.15	.073
Women:	" " " smallest interval produced " " "	—.11	.065
	" " " m.v. on O. " " "	+.27	.061
	" " " C.E. on O. " " "	+.11	.065
	" " " m.v. on V. " " "	+.51	.048
	" " " C.E. on V. " " "	—.07	.065

It will be recalled that in order to be satisfactory a coefficient "should be perhaps three to five times as large" as its probable error. This rule liberally applied to our results leaves us the coefficients +.33 and +.51 both of unquestionable reliability. These coefficients represent the correlation between pitch discrimination and the average mean variation in singing the intervals, for men and women respectively.

The test of 1910

A series of musical tests, given by the writer in the Iowa Psychological Laboratory, during November and December of 1910, included one on Accuracy in Reproducing Tones. There were ninety men and one hundred and seven women, members of the elementary psychology classes who took this test.

The apparatus besides the tonoscope consisted of five large forks with pitches as follows: 128, 256, 320, 384, and 512 v.d. The experimenter instructed the observer to take the 256 v.d. fork, strike it gently, bring it to his ear, listen carefully, and then to reproduce the same pitch. This he repeated with fork 256 v.d. Then taking the 320 v.d. fork he proceeded as described. The last four forks were gone over five times in this manner, which gives ten trials on each tone, forty trials in all. The test is thus very simple, the reproduction of four tones (two successive trials on each) which are at the same time natural musical intervals: major third, fifth and octave. No restrictions were placed upon the observer in the matter of humming or singing with the standards. As the fork was in his hand he sang with it or after it as seemed best to him. About one-half the observers preferred to take the fork away from the ear before beginning to sing. The men sang the tones one octave below

the pitch of the fork. When it was difficult for them to commence doing this, the 128 v.d. standard was used for orientation.

The results for this series of 8,000 reproductions are given in Table IX. The notation is the same as in Table VIII. The test of 1910 was complicated by the factor of natural musical intervals, it was also considerably shorter and simpler than the one of 1913 but in comparing it with the latter we find the results in practical agreement on some points.

(1). There is a uniform tendency for the majority of observers to sing sharp. Here again the tendency appears to be greater for women than for men, the G. C. E. for men being + .26 v.d., for

TABLE IX. Accuracy of singing: test of 1910

90 men	128 v.d.	160 v.d.	192 v.d.	256 v.d.
Ave. m.v.	1.42	1.36	1.43	1.79
% + C. E.	47	63	51	63
% — C. E.	53	37	49	37
Av. C. E.	1.62	1.62	1.72	2.70
G. C. E.	+ .26	+ .65	— .06	+1.06

107 women	256 v.d.	320 v.d.	384 v.d.	512 v.d.
Ave. m.v.	1.89	2.15	2.07	2.83
% + C. E.	81	90	84	86
% — C. E.	19	10	16	14
Av. C. E.	2.59	3.91	3.47	5.90
G.C.E.	+2.39	+3.36	+3.11	+4.76

women + 2.39 v.d., a difference of 2.13 v.d. as contrasted with 1.30 v.d. in the previous measurements. In the test of 1910, as mentioned, the men and women used the same forks, the men singing the standards one octave low. Therefore the tendency for men to sing less sharp than women in the 1913 experiments can hardly be attributed to a timbre or sound volume difference between the sets of forks. The men are much more evenly divided between the sharping and flatting tendencies than the women, for example on 256 v.d. the one tone which both sexes had in common, the percentages in favor of sharping are 63 and 86 for men and women respectively. (2) The average constant error (arithmetic) on 128 v.d. is slightly larger in 1910, 1.62 v.d. as against 1.54 v.d. The mean variation for 128 v.d. are 1.42 v.d. (1910) and 1.54 v.d. These differences are rather slight. (3) Men and women sing their one common tone (256 v.d.)

with equal accuracy: m.v. 1.79 v.d., Av. C. E., 2.70 v. d. (men) to m.v., 1.89 v.d., Av. C.E. 2.59 (women). It would seem from a comparison of available norms for voice range in the sexes (Helmholtz (9) and Zahm (27) that 256 v.d. should be about as high for men as it is low for women, and that it is well within the average range of both. We have here therefore a confirmation of our previous conclusion, *i.e.,* that men and women sing with equal accuracy vibration for vibration. However the errors in this case under consideration (1910) are much larger than the results of Series VI would lead us to expect. This is true of all the tones sung by the women and renders them incomparable with the previous results.

Recommendations toward a standard test

The recommendations which follow must be considered as preliminary and as applying simply to the two measures of singing ability considered throughout this study, *i.e.,* the ability of the voice to reproduce pitch, and the ability to produce voluntarily small changes sharp or flat in the pitch of the voice.

1. The two factors may be tested together with advantage. They are closely related phases of the same thing. Neither of them should be taken in combination with such factors as accuracy of tone memory, or judgment for musical intervals.

2. Use a graded series of standard tones similar to that commonly employed in testing for pitch discrimination. Such a series has obvious advantages over the use of a single standard; (1) If several observations are to be made at a single sitting the effects of practice are not so great. (2) The small pitch intervals make clear to the observer what he is expected to do with his voice. (3) The variety of standards (and hence degrees of difficulty) reduce monotony and fatigue. A graded series furthermore has advantage over any other series: (1) it keeps the test comparatively free from complication with the singing of musical intervals, and (2) when the standards represent small steps of pitch difference the observer discriminates more carefully and is not so likely to be satisfied with a mere approximation.

3. Use tuning forks for standards. They are very easily manipulated, are not subject to certain sources of error commonly met in the control of reeds, pipes and strings, and are readily arranged

into a graded series as recommended above. Any disadvantage, if indeed it may be so called, from the standpoint of the purity of the fork tone seems more than compensated for in having a definable quality and a standard on which all observers are equally unpracticed.

4. Begin with the largest pitch increments and proceed to the smallest and then in reverse order back to the largest. This will economize effort, provide the best practice, and help to control the attention. For general testing ten intervals representing as many degrees of difficulty, ranging from 0-30 to 0-.5 are not too many. For extensive testing of one observer or in working with highly practiced observers the increments which are distinctly above the threshold for pitch discrimination may be omitted.

5. Give the tones in pairs, presenting the variant tone immediately after the reproduction of the standard, thus securing a rapid adjustment which favors discrimination in the kinaesthetic sensations from the larynx. As an alternative procedure the two tones might be presented in immediate succession as in the pitch discrimination tests, the observer carrying the standard in mind while listening to the variant, and then singing them in quick succession.

6. Control conditions: (1) The forks should be presented before resonators which are some distance from the observer and care must be exercised to present them with uniform intensity. (2) The observer should use a medium volume of voice in singing the tones, (3) The experimenter should select the vowel to be sung and insist on a good quality. (4) If time intervals are used between standards and reproductions they should be short, not longer than two seconds at most. (5) Time intervals should be introduced between pairs of tones. These should be at least 2 seconds in length. Longer intervals would doubtless be better as the voice could the more easily be kept out of a "rut" in reproducing the standard. (6) Secure effort on the part of the observer who is too easily satisfied with his own performance.

Our test is one of motor control. As a musical test it bears the same relation to the motor side as pitch discrimination does to the sensory side. In fact it is in a practical way the motor pitch discrimination of the singer, and as far as singing is concerned it is more important than simple sensory pitch discrimination.

SUMMARY OF CONCLUSIONS

Among others the following general conclusions may be gleaned from the foregoing experiments.

1. The human voice is about equally accurate, in terms of vibration, at all points well within its range; therefore, the high tones are sung relatively (per cent.) more exactly than those which are low.

2. A strong standard tone (especially with low forks) is reproduced as decidedly lower than a weak standard.

3. The voice can most easily reproduce pitch for those standard tones which have a rich timbre, such as the organ tone.

4. Measured in terms of average error the voice is less accurate when its volume is large.

5. Vowel quality affects the accuracy of vocal reproduction of tones. The *"i"* (as i in machine) is reproduced the highest, *"o"* the lowest, and *"a"* occupies a middle position.

6. Men and women sing in their representative ranges with equal accuracy vibration for vibration of error.

7. Women show better relative voice control than men, if judged on the basis of their mean variation.

8. With women there is a general tendency to sing sharp. Men are about equally divided in this regard, sharping however being slightly more frequent.

9. The average error of the voice in reproducing a tone given by a fork is 1.5 v.d. for men at range 128 v.d., and 1.5. v.d. for women at 256 v.d. in a representative group of students.

10. A small perceptible pitch difference between two tones is overestimated in the signing.

11. The average minimal producible change of the voice for men at 128 v.d. is about 5.5 v.d., and for women at 256 v.d. it is 3.5 v.d.

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THE EFFECT OF TRAINING IN PITCH DISCRIMINATION

BY

FRANKLIN ORION SMITH

CONTENTS

The present investigation forms a part of a series of researches in the Iowa laboratory,[1] upon the tonal hearing. The problem was to determine the effects of training in tonal hearing, considering age, sex, musical education, general intelligence, and kinship.

The investigation consisted of a preliminary training series, a ten days' practice series and the correlation of the results with those of other researches. The experiments were conducted in the University and in the public schools of Iowa City and Cedar Rapids in 1908-1912.

Method of procedure

The tuning forks and accessories which were employed in this research are fully described by Professor Seashore in his report for the American Psychological Association on the standardizing of pitch discrimination tests.[2] The experimental precautions, both subjective and objective, were observed as set forth in that report. The only change made in the apparatus consisted in using two resonators instead of one, which is a decided improvement because one resonator alone does not speak sufficiently well at the extremes where increments as large as 23 or 30 v.d. are used. The methods of procedure recommended in the above named report were followed, as

[1] The writer wishes to acknowledge his manifold indebtedness to Professor Seashore for his supervision and coöperation, which have made this research possible. To Dr. Mabel C. Williams and to companions in research who are working upon related problems in the laboratory, he expresses his grateful appreciation for assistance.

[2] Psychol. Monog. No. 53.

described on pages 39-43 of that report. The "heterogeneous" method was used in all preliminary experiments and with unclassified groups. This consists in presenting the increments 30, 23, 17, 12, 8, 5, 3, 2, 1, and .5 v.d. in the order named a number of times and finding at what level in that series the threshold falls in from ten to twenty trials. The mean variation of the records for all such sets is then computed by the method described on page 42 of the above named report, as follows:

"For ordinary work we therefore recommend as a measure of variation in the record the use of the mean variation (m.v.) computed as follows: Regard the difference between successive steps as equal psycho-physic steps and, with the increment which is nearest to the median as a base, multiply the number of cases which are one step from this base by 1, the number that are two steps away by 2, the number that are three steps away by 3, etc.: divide the sum of these products by the total number of cases (sets)."

The homogeneous method is the ordinary method of right and wrong cases or constant stimuli, counting the threshold at 75 per cent. correct cases. This method was used in dealing with individuals or groups formed on the basis of preliminary tests.

The preliminary training consisted of two tests which are designated as the first and second preliminary tests respectively. The observers consisted of pupils in the elementary and high schools, and students in the University. The ages vary from nine years to maturity. Most of the observers were unmusical in the sense that they had received no special training in music. These tests were made in the schoolrooms under good conditions. The temperature and ventilation were regulated by automatic systems (except in two small grade schools). The regular teacher remained in the room during the experiment maintaining normal conditions of order and school spirit. These general conditions did not differ materially among the schools nor among the different rooms of the same school. The tests were carried on in the morning between nine and twelve o'clock, each test lasting twenty to twenty-five minutes.

Since it was not practicable in all cases to employ the homogeneous method, all the group tests were made by the heterogeneous method. In figuring the results the nearest whole vibration (except 0.5 v.d.) was taken. The increments in the series of tones used (0.5, 1, 2, 3, 5, 8, 12, 17, 23 and 30 v.d.) are referred to as units and are con-

sidered equally difficult to distinguish. That is, 23 to 30 v.d. is assumed to be as difficult for one whose threshold is 23 v.d. as 1 to 2 v.d. is for one whose threshold is 1 v.d.

In case of defective hearing the pupil was seated where he would be certain to hear; or, if the deafness was serious, he was excused from the test. The rhythm of the work period was not so easily controlled. The tests were comparatively short and every effort was made both by the experimenter and the attending teacher to keep the effort up at a high pitch throughout the test. Indifference is perhaps the largest source of error in the few cases where it was manifest. This could be recognized directly at the time of the test and usually also by the distribution of errors in the records.

One of the most striking and yet perplexing facts about pitch discrimination is that there is often no relation between the feeling of certainty and the correctness of the judgment. The judgment is often based upon a clear illusion. This illusion of hearing in the case of wrong judgment aids much in the encouragement to sustained effort.

Anticipatory judging is a fruitful source of errors. Under the influence of expectant attention the observer anticipates the second tone the moment he hears the first. The experience is analogous to the illusion of lifted weights. With a strong expectation of hearing the second tone high, or low, the organism is set to make the appropriate response and this has marked influence upon the judgment. Closely related to anticipatory judging is the tendency to compare the present tone with the preceding pair. In fact this tendency often leads to anticipatory judging especially when the first tone of the present pair is compared immediately with the last tone of the preceding pair.

The confusion of pitch and intensity is a troublesome source of error, particularly with unpracticed observers. Making the tones actually objectively equal in intensity does not always allay the difficulty as disturbing associations may tend to create confusion. High tones are intrinsically louder than low tones. A slight difference in intensity is often interpreted as a difference in pitch.

In computing the characteristic figure of a record it is necessary to take account of internal evidences and make a "correction" as is explained in the report of this test referred to above, pages 45-48. This must always be a matter of "good judgment" and can not be

done mechanically. Certain factors may however be quite clear and exact. The distribution of the records in the heterogeneous test with respect to the operation of the laws of chance is one of the most telling. A record of, *e.g.*, 8 v.d. may on examination of the distribution of the errors be found to contain indisputable proof of a threshold of 2, or 1, or .5 v.d. as the case may be.

Sometimes when a source of error has been noted a study of the distribution may show where it operated and where it did not operate. A small mean variation, *e.g.*, 1.0 or less is almost certain proof of the reliabliity of the actually computed median. The study of the internal evidences therefore has its principal use in cases showing a large mean variation. All our records were examined with reference to internal evidence of error in the computed median and, it must be frankly admitted, wherever such evidence was found the correction was made. All the records here used in the heterogeneous method are therefore "corrected" records. Fig. 1 shows, it will be seen, that the tendency of the correction is to lower the record and that most of the corrections are made for those who have poor records.

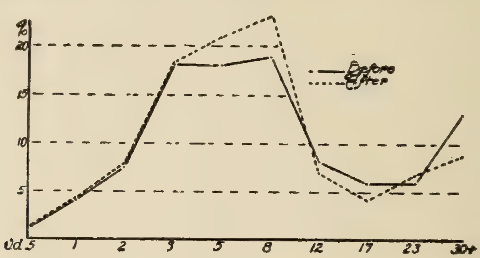


Fig. 1. Distribution of 476 pupils for one day's practice before and after the records had been corrected.

The effect of instruction

As a preliminary to the training series two tests of about 25 minutes each were given to 1980 pupils by the heterogeneous method in their regular class divisions. In the first period the test was begun without any explanation beyond what was necessary to direct them about reporting "higher" or "lower". The second period was opened with simple and diversified explanations and illustrations of what pitch is. This explanation was based upon a previous study of the kinds of difficulties encountered. Pitch was differentiated

from intensity, duration, volume, timbre, etc. in familiar talk and by different instruments.

Unfortunately the two factors of instruction and experience, or direct observation resulting in a growing familiarity with the problem, are not isolated. We have simply the records for the two periods and must interpret the gain as due to both of these factors, which are, of course, inseparably associated.

To facilitate comparison the observers were divided on the basis of these tests, into A, B, and C grades in accordance with the possession of a good, medium, or poor ear. Grade A includes those who

TABLE I. Distribution of those who improved in the preliminary test

	30	*23*	*17*	*12*	*8*	*5*	*3*	*2*	*1*	*0.5*	*A*	*B*
30+	11	11	10	15	6	3	2	1	0	0	59	
	23	*17*	*12*	*8*	*5*	*3*	*2*	*1*	*0.5*			
30	13	13	11	17	7	3	0	0	0		64	11
	17	*12*	*8*	*5*	*3*	*2*	*1*	*0.5*				
23	13	12	15	2	3	0	0	0			45	24
	12	*8*	*5*	*3*	*2*	*1*	*0.5*					
17	17	38	14	4	1	1	1				76	36
	8	*5*	*3*	*2*	*1*	*0.5*						
12	25	30	16	3	1						75	55
	5	*3*	*2*	*1*	*0.5*							
8	103	43	15	10	0						171	101
	3	*2*	*1*	*0.5*								
5	126	35	8	8							177	159
	2	*1*	*0.5*									
3	122	45	13								180	197
	1	*0.5*										
2	44	7									51	177
	0.5											
1	9										9	147

Italics designate increments; the other figures give the number of cases for each of the respective degrees of improvement; thus, of those who had a record of 30 + in the first test, 11 went to 30 —, 11 to 23, 10 to 17, 15 to 20, 6 to 8, 3 to 5, 2 to 3, and 1 to 2 in the second test. *A* shows the total number of cases at each increment in the first test; *B* same in the second test.

hear differences of less than 3 v.d.; grade B those who hear differences of 3 to 14 v.d.; and grade C those who hear differences of 14 to 30 v.d. or above.

The records show that 54 per cent. made no improvement in the second test; 46 per cent. of all observers made better records in the second preliminary test than in the first. The amount gained varies from 1 to 8 units. The average amount gained varies from 3.8 v.d. at nine years of age to 0.3 v.d. at maturity.

Table I analyzes the distribution and the amount of gain by the

cases (46 per cent.) which improved with the instruction. Of the 46 per cent. who improved, only 7 per cent. changed from grade C to grade A in the second test. Of the 425 pupils (22 per cent.) who improved and were in grade B at the beginning, 255 (60 per cent.) changed to grade A in the second test. Measured by the first test 26.5 per cent. of those who improved were in grade A. Measured by the second test 70 per cent. were in grade A. Of the changes to grade A, 96 per cent. were from grade B; and 91 per cent. of the changes to grade B were from grade C.

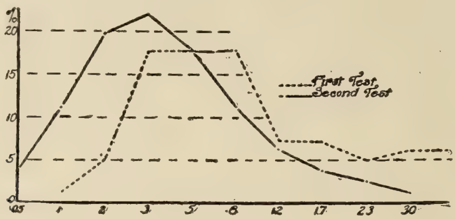


Fig. 2. The effect of instruction. Distribution of 907 pupils who made improvement from the first to the second preliminary test.

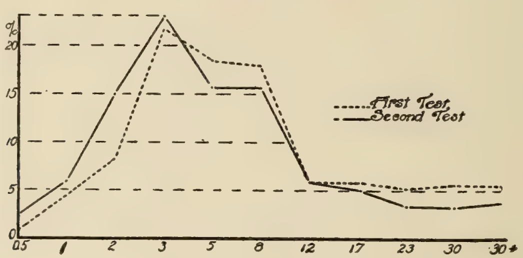


Fig. 3. Distribution of entire group, 1980 cases, in preliminary tests.

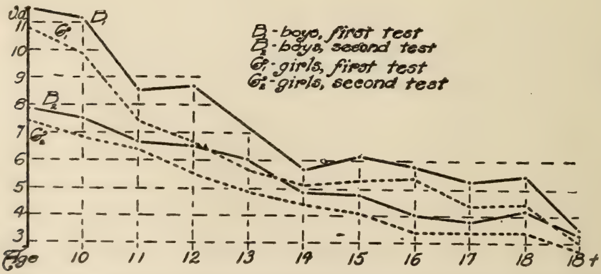


Fig. 4. Distribution of improvement in the preliminary tests by age and sex, 417 boys and 490 girls.

The effect of the instruction and experience thus gained from the first to the second test is shown in Fig. 2 which represents only the 46 per cent. of cases in which improvement was made. Fig. 3 shows the effect upon the whole group of 1980 cases. Fig. 4 shows the distribution of improvement by age and sex.

TABLE II. Distribution of forty-seven out of fifty-four university students who improved with individual instructon

	23	*17*	*12*	*8*	*5*	*3*	*A*	*B*
30	1	1	1	2	1	0	6	
	17	*12*	*8*	*5*	*3*	*2*		
23	0	0	1	1	0	0	2	1
	12	*8*	*5*	*3*	*2*	*1*		
17	0	0	4	1	1	2	8	1
	8	*5*	*3*	*2*	*1*	*0.5*		
12	2	2	1	1	0	2	8	1
	5	*3*	*2*	*1*	*0.5*			
8	3	6	2	1	0		12	5
	3	*2*	*1*	*0.5*				
5	4	1	4	2			11	11
			Below 5					28

Notation and plan of this table same as in Table I.

A similar test of the effect of instruction was made in a class of 200 adults. After two preliminary tests, one heterogeneous and one homogeneous, the poorest one-fourth of the group were taken and instructed individually as to the actual nature of pitch hearing. An effort was made to find out what particular difficulties they were encountering, and explanation and illustration were based progressively upon this information. As a class these had made but little improvement in the second preliminary test, both the first and the second having been given "without instruction". But as a result of this personal instruction all but 7, *i.e.* 47 out of the 54 made rapid improvement. The change in the record for the group is shown in Fig. 5 by giving the distribution at the beginning and at the end of the period of individual instruction. The distribution of the gain is analyzed in Table II.

The fact that these were adults familiar with the class room and trained in many psychological experiments, yet made such marked response to the instruction and individual help, doubly emphasizes the importance of thoroughness and individual attention in the instructions if the records are to be entirely reliable.

One of the best experimental proofs that we have showing the efficacy of individual care and instruction is found in the un-

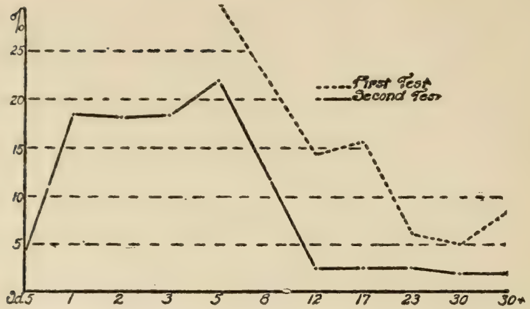


Fig. 5. Distribution of 54 university students in individual tests (Table II).

published experiments of Dr. H. S. Buffum, which have been summarized in the above mentioned Psychological Association report by Professor Seashore, as follows:

Dr. Buffum experimented on twenty-five eighth grade pupils in a grammar school room. He first made a fifteen minute individual test of each pupil and classified them on this basis into three groups with modes at 3, 8, and 17 v.d. respectively. The object was twofold: (1) to determine the effect of practice and (2) to determine the success of the preliminary examination. For this purpose he gave them twenty forty-five-minute periods of training.

The results show (1) that for no group is there any evidence of improvement with this practice, and (2) that all except two children remained throughout the whole practice series within the group to which they had been assigned. Of these two, one who had been assigned to group III was immediately found to belong to group I as there had been a failure to understand the preliminary test; and the other, although retained in group II, proved really to be near the dividing line and could have been classified in group III. Evidently the physiological threshold had been reached in twenty-four of the twenty-five cases in the preliminary test."

In Dr. Buffum's experiment the fifteen-minute preliminary classification was so efficient as practically to eliminate poor records due to ignorance of the test.

The significance of instruction is further proved by the records in successive classes in the university for a period of years. It is found that the average record has improved slightly from year to year. There is no reason for believing that this is due to anything

but improved skill and technique and increased care in the instructions and charge to those about to be examined.

In the above records we have conclusive evidence that effective instruction is of the greatest importance in making records on pitch discrimination. It is not a poor ear, but ignorance that accounts for the bulk of poor records in a first test. Those who made a fine record in the first test are, of course, not subject to this source of error; and those who have poor records but show no improvement after instruction or prolonged training may also be free from this source of error. It is a safe rule to say that all tests should be preceded by efficient instruction; if this can be made individual, so much the better; and all who show poor records must be subjected to more intensive and searching instruction before the record can be accepted for serious purposes.

The effect of practice

The first of the two extensive experiments in practice was a series of group tests by the "heterogeneous" method covering a period of ten days. The second was a series of individual tests on adults by the "homogeneous" method. In addition to these certain special training series will be described.

The group tests were made on 476 pupils (215 boys and 261 girls) in two elementary schools selected from those in which the preliminary tests had been made. These practice tests were conducted in the same manner and under the same conditions as the preliminary tests except with regard to instruction. Each test was preceded by a brief warming-up exercise in which the pupils answered orally. This also helped to keep interest alive. A short rest period was taken at the middle of each test. At this time opportunity was given the pupils to ask questions about the test.

Running parallel with the class tests were certain individual tests which were carried on in the afternoon following a given set of class tests. At the noon intermission the records of one or two grades were checked up and pupils whose threshold for that day was between 20 v.d. and 30 v.d. were given individual practice. The object of these individual tests was to give special assistance to backward pupils, aiding them to distinguish different tone qualities and to form right habits of attention. These tests include 71 boys and 35 girls constituting the poorest in the group tests.

With regard to the general musical preparation of these pupils it may be said that music was taught systematically in all the grades, and that the schools were provided with Victor graphophones in which high grade selections were played regularly.

For comparison the cases under observation may be divided as follows: Group I, those who made no improvement either with instruction or practice; Group II, those who made no improvement in the practice; Group III, those who made little (1-3 v.d.) improvement in the practice; and Group IV, those who made marked improvement (3 v.d. +).

The records of these practice series on children are set forth in Tables III-IX and Figs. 6-10. Table III gives the daily average threshold for the twelve days of training by ages, section A showing those who do not improve with training and B those who do improve with training. Table IV gives the daily average threshold for those who improve with training regardless of age for the four

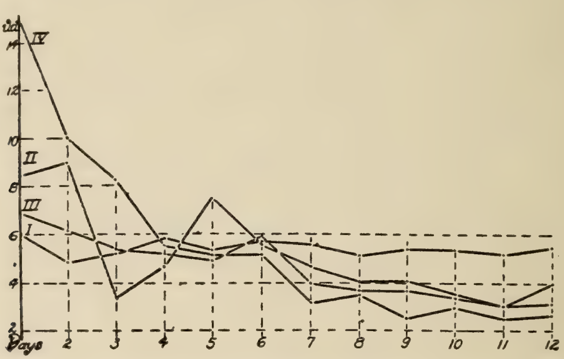


Fig. 6. Daily average, by groups, of those in the practice series (Table VI).

Fig. 7. Daily average by sex (Table V).

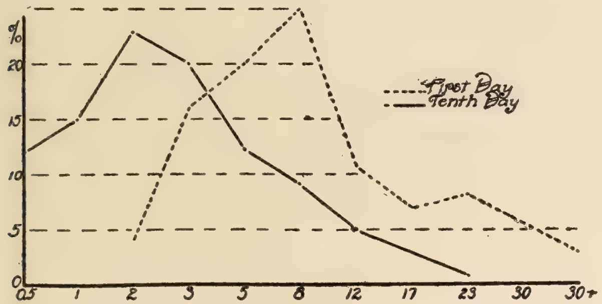


Fig. 8. Distribution of 270 pupils who improved with practice (Table VI).

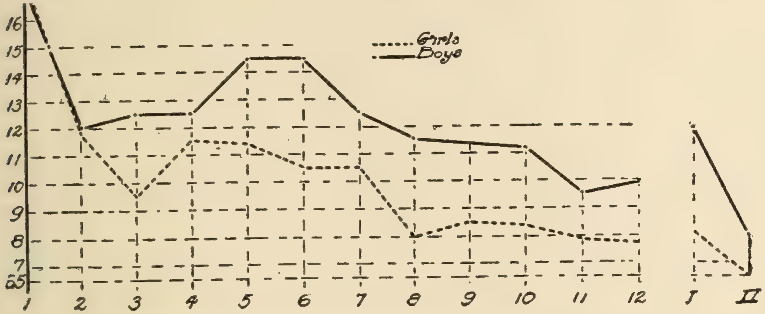


Fig. 9. Daily average record of those who were given special individual help (Table VII).

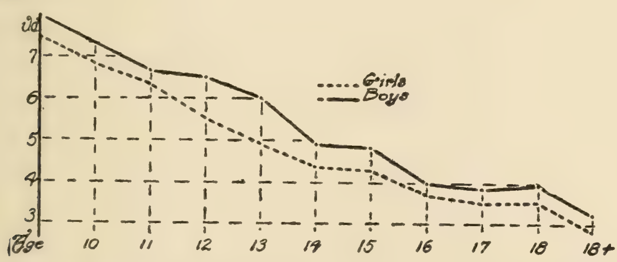


Fig. 10. Comparison by ages of the average (median) abilities of boys and girls.

groups. Table V gives the averages of the same separately for the boys and girls, Groups II, III, and IV combined. Table VI gives an analysis of the distribution of those who improve with practice. Table VII (Fig. 9) contains the record of those who were given individual tests or help during the practice, showing the daily record and the record of two individual tests in the average for the group. Table VIII gives a comparison of the mean variations with reference to sex and age. Table IX gives the distribution of those who attain the approximate physiological threshold in different days of the practice.

TABLE III. Daily average threshold, by age

A. Group I: those who made no improvement with training

Age	1	2	3	4	5	6	7	8	9	10	11	12	Number
9	6	6	6	6	8	7	8	10	9	8	7	8	29
10	6	6	4	7	8	8	6	7	7	8	6	8	31
11	8	4	6	7	5	7	6	7	5	6	7	8	47
12	8	8	8	5	7	8	7	7	8	8	8	5	23
13	5	5	5	5	4	5	5	5	4	4	5	5	29
14	6	5	5	5	4	4	5	4	4	4	4	5	32
15	5	6	5	5	4	4	4	4	4	4	4	4	15

B. Groups II, III, IV combined: those who made improvement

Age	1	2	3	4	5	6	7	8	9	10	11	12	Number
9	7	5	5	5	4	4	4	4	4	3	3	2	16
10	7	7	5	5	4	4	5	3	3	3	3	3	43
11	10	10	8	7	8	5	5	5	6	5	4	5	43
12	7	5	4	3	3	3	2	2	2	2	2	2	53
13	8	8	5	5	6	4	4	4	3	2	2	2	51
14	7	7	5	5	5	5	4	3	4	3	3	3	34
15	7	8	7	5	5	5	4	3	3	3	4	3	30
Ave.	7.5	7.1	5.7	5.0	5.0	4.3	4.1	3.4	3.5	3.0	3.0	2.9	
% gain		9	30	15	0	15	4	15	0	9	0	2	

TABLE IV. Daily average by groups

	1	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	Number
Group I	5.9	4.8	5.2	5.8	5.1	5.5	5.4	5.0	5.4	5.2	5.1	5.4	206
Group II	8.5	9.0	3.3	4.6	7.6	5.3	4.7	4.0	4.0	3.5	3.0	5.0	52
Group III	6.9	6.2	5.4	5.1	5.0	5.8	4.5	3.5	3.5	3.4	3.1	3.0	172
Group IV	15.0	10.0	8.2	5.5	5.1	5.1	3.1	3.4	2.5	2.9	2.6	2.7	46

TABLE V. (Fig. 7) Daily average by sex for Groups II, III, and IV

	Number	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*
Boys:	215	8.1	6.5	6.3	5.8	5.6	5.2	5.3	5.2	5.0	4.6	4.6	4.7
Girls:	261	6.3	5.3	5.2	5.2	4.9	4.8	4.7	4.7	4.5	4.0	3.5	4.5

TABLE VI. Distribution of those who improve with practice

	30	*23*	*17*	*12*	*8*	*5*	*3*	*2*	*1*	*0.5*	*A*	*B*
30+	0	1	1	2	2	1	0	0	1	0	8	0
	23	*17*	*12*	*8*	*5*	*3*	*2*	*1*	*0.5*			
30	2	3	2	4	1	2	1	1	0		16	0
	17	*12*	*8*	*5*	*3*	*2*	*1*	*0.5*				
23	4	5	4	4	2	1	1	0			21	3
	12	*8*	5	*3*	*2*	*1*	*0.5*					
17	5	7	3	2	1	0	0				18	8
	8	*5*	*3*	*2*	*1*	*0.5*						
12	8	10	8	3	1	1					31	14
	5	*3*	*2*	*1*	*0.5*							
8	14	26	15	6	6						67	25
	3	*2*	*1*	*0.5*								
5	15	26	10	4							55	33
	2	*1*	*0.5*									
3	12	16	14								42	55
	1	*0.5*										
2	4	8									12	59
						Below 2						73

Notation and plan of this table same as in Table I.

TABLE VII. Daily average record of those who were given special individual help

Days	1	2	3	4	5	6	7	8	9	10	11	12		
Boys:	17.3	12	12.5	12.5	14.5	15.5	12.5	11.4	11.3	11.2	9.5	9.8	*12.*	*8.*
Girls:	17.7	11.8	9.5	11.5	11.4	10.5	10.5	8.	8.5	8.4	7.9	7.8	*8.2*	6.5

Italics, average record on the first and the second individual tests respectively.

TABLE VIII. Average mean variation from the individual records in the preliminary and final tests.

	Boys (215)		Girls (261)	
Age	Prelim.	Final	Prelim.	Final
9	1.82	1.93	1.99	1.97
10	1.66	1.81	1.76	1.60
11	1.63	1.71	1.68	1.82
12	1.70	1.53	1.51	1.68
13	1.47	1.52	1.54	1.60
14	1.45	1.48	1.65	1.65
15	1.64	1.59	1.53	1.38
Total	1.61	1.65	1.65	1.69

TABLE IX. Distribution of those who reach the approximate physiological threshold on different days of practice.

Days	1	2	3	4	5	6	7	8	9	10
Per cent.	6	8	9	9	9	13	12	13	8	7

Of the 476 children 206 (43%) fall in Group I; *i.e.,* so far as the instruction and practice are concerned, these made no improvement that could be traced in the records, due allowance being made for daily variable errors. The number of those who do not improve with practice is relatively greater for the younger than for the older children.

Classifying these 206 on the basis of record into Grade A, those whose threshold is 4 v.d. or less; Grade B, those whose threshold is between 4 v.d. and 14 v.d.; and Grade C, those whose threshold lies above 14 v.d., we find 40 per cent. in Grade A, 51 per cent. in Grade B, and 9 per cent in Grade C. Of the 270 cases (57 per cent.) which show improvement with practice 19 per cent. are in Grade A, 64 per cent. in Grade B and 17 per cent. in Grade C.

Relatively the largest number of cases of improvement occur among those who start out with a very inferior record. This can be shown by comparing the distribution of cases which make improvement for each of the increments as set out in Column A, Table VI with the normal distribution of thresholds for the entire group.

Of those who did not improve ten were unable to hear any of the increments used and judge as required. It was however found upon

making private examination of the seven of these who were available that they could hear tone differences. Two of these could distinguish between A and B on the piano. Two of them seemed unable to grasp the concepts "high" and "low" with reference to the naming of pitch. One of these—a scatter-brain—could, however, sing a half-tone correctly when played on the piano. The other—retarded about five years—could sing a fifth fairly accurately with the piano. Three were able to imitate a pitch difference in the forks of 3 v.d. by singing enough to show whether the second of the two tones was sharp or flat. The other three were, unfortunately, not available for special tests. Thus, of the 476 cases not a single case of so-called tone deafness was found.

The last line in the footings of Table III, B shows that the gain of those who do improve is most rapid in the first part of the training series, 54 per cent. of the gain being made in the first three steps. The further analysis of these figures in Table IV, illustrated by Fig. 6, shows that this principle is true for all three of the groups which show improvement.

All the observers included in Table VII took the first individual test which occurred on different days, from the third to the seventh day. Most of these tests were given early in the practice series. The second test began on the fifth day and extended over the remainder of the practice series. Only 26 boys and 9 girls needed to take this test. A very few were given a third test near the end of the practice but the results were not included in the table. Not only does the individual test yield a lower median than the group test in a majority of cases, but the individual test often influences the later results of group practice. In this experiment 6 boys and 4 girls made immediate and permanent improvement after the first individual test which it will be remembered was accompanied by instruction. In one case the gain was from 30 to 9 v.d.; in another from 23 to 5 v.d. and in a third from 24 to 10 v.d. In some cases improvement did not begin until after the second test, and in the case of 29 boys and 13 girls no improvement was made. Of these only 2 (both girls) made better records in the individual tests.

The average amount of improvement for all cases at each increment decreases with the diminishing of the increment. This is seen in Table VI, and may also be seen graphically in Fig. 8. It must be remembered that this figure does not represent the whole group but only those who improved.

The series is not long enough to guarantee that any or all reached the physiological threshold.[3] The main difficulty in determining this lies in the fact that persons often come to a "plateau" in the record which is due to some motive or condition which may be removed by instruction or training. This, however, gives trouble only when it continues to the end of the training series. Classifying the cases roughly on internal evidences of the records we find that what may be approximately the physiological limit is reached in successive days as set forth in Table IX. From the variations in the records it is quite clear that the data in this table are quite problematical. To get the actual physiological threshold it is necessary to have more favorable conditions for isolation of the observer and the elimination of disturbances. Undoubtedly there may also be several who remain on a "cognitive" plateau throughout this series and would improve under the proper impetus. Yet, due allowance being made for these sources of error, the table shows that there is a "rapid maturing" in this training; 6 per cent. reach their bed-rock level on the first day, 8 per cent. on the second, 9 per cent. on the third, etc.

After the preliminary tests the number who reach the approximate physiological threshold increases gradually to the fourth day. On the fifth day the number increases suddenly from 24 to 41 (9 per cent. to 15 per cent.) and then gradually decreases to the eighth day after which there is a rapid falling off to the tenth day. (Table IX). The results show that 47 per cent. of those who improve reach the approximate physiological threshold by the fifth day of practice.

The mean variation as given in Table VIII conveys three significant items—the result of practice, the variations with age, and the

[3] The term is here used in the sense defined by Seashore (3) page 49-50. "*The Cognitive* vs. *the Physiological Threshold.* In sensory discrimination of this sort we may speak of two thresholds: the physiological, which is set by the limits of capacity in the end organ; and the cognitive, which is set by cognitive limitations. Theoretically we always aim to reach the physiological threshold, but practically we often fall short of this and find a cognitive limit; *i.e.,* a higher threshold due to lack of information, best form of attention, interest, effort, etc.; or to disturbances of some sort. Usually inspection of a record or observations made in the test enable us to tell whether or not we have reached the physological threshold. It cannot be judged by a single rule, although a small m.v. and a well defined mode are pretty sure indications. This distinction is of greatest importance in classification, and in the theory of training."

variations with sex. It must be borne in mind that the unit of the m.v. is not the vibration but the increment, as was described above. That is, the increments increase in a geometric ratio of the second order; therefor, *e.g.*, the increment 17-23 v.d. counts one unit just as do the increments 5-8 v.d. or 1-2 v.d. It follows that as the threshold is lowered the mean variation unit remains relatively constant. Equal power of application of those who have high and those who have low thresholds should therefore show in about equal mean variations; and, conversely, unreliability in judgment will show in increased mean variation equally for the one who has a fine ear and the one who has a poor ear.

The mean variation is slightly larger in the final training test than in the preliminary. The difference is not large—only .04 units—but it is fairly constant for all ages and for both sexes. This is rather remarkable as, in the nature of the test, one would look for evidences of increasing familiarity in the lowering of the mean variation. On the other hand the fact that the procedure does not reduce the mean variation is a most telling proof of the elemental nature of the test. The test is so stripped of conditions for variablity that it is possible to be as consistent in the preliminary trial as in trials after practice.

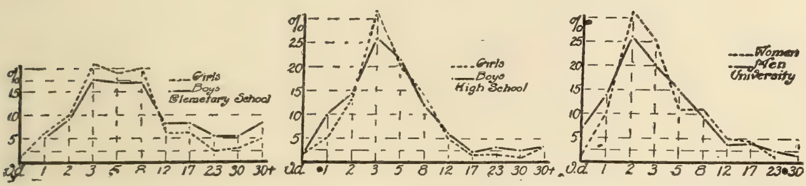
The variablity is a trifle larger for girls, both in the preliminary and the final tests. This is true for all the ages except 12 and 15 in the preliminary and 10 and 15 in the final. Were it not that this has a bearing on the much mooted question of sex difference on this point and that the data here given represent such a large number of cases (1980 in the preliminary and 476 in the final) no significance would be attached to this difference. The second decimal figure is of doubtful value in an average of this kind and, as stated, the variation is in one direction for five ages and in the opposite for two both in the preliminary and the final. On the whole our interpretation is, therefore, that practically there is no significant difference in the variability of the boys and the girls in pitch discrimination.

There is a general, though not great, tendency for the mean variation to decrease with age. This is the measure of growing reliability with age which we are accustomed to find in records of this kind.

In this practice series in the elementary schools there are two

items that count distinctly in favor of the girls. One is that of the 215 boys and 261 girls who took the practice series, 71 boys and 35 girls were considered, on the same basis, poor enough to need individual instruction and drill. This is a distinct mark of superiority in the girls. The other is that the girls in the training series, quite uniformly for all ages, have a lower threshold than the boys by from one to two vibrations. (See Table V, and Fig. 7).

This superiority of the girls over the boys is evidenced also in the preliminary tests with remarkable uniformity as is seen in Fig. 10 where a fairly constant difference is maintained throughout all the ages. The same fact is illustrated from another point of view for the elementary school in Table X and Fig. 11. This difference, however, disappears when we come to the higher ages. Fig. 12, for the high school, and Fig. 13 for the university, based on Table X reveal no recognizable superiority of either sex in the preliminary tests.



Figs. 11, 12, 13. Variation with sex and age. Based on preliminary tests in the elementary schools.

A comparison of pitch discrimination for different ages in the preliminary tests is given in Table X. This shows that the order of superiority is,—university students, high school pupils, and elementary pupils, the respective modes being roughly 2, 3, and 4 v.d. This comparison is however not quite fair, inasmuch as longer tests were given to the university students than to the high school pupils and longer to the high school pupils than to the elementary pupils; and the longer the test the more favorable the results tend to be. As will be shown later, this, together with the better control of experimental condition among the older pupils, may be ample to account for the differences here shown, so that, under equally good conditions of testing, there would probably be no evidence of variation with age.

In Table XI we see that at the age 9 the cases are about equally distributed in the three grades. Grade B remains about

constant for all ages; but the number of cases in Grade A decreases with age as the number of cases in Grade C increases. Fig. 14 shows a comparison for age only.

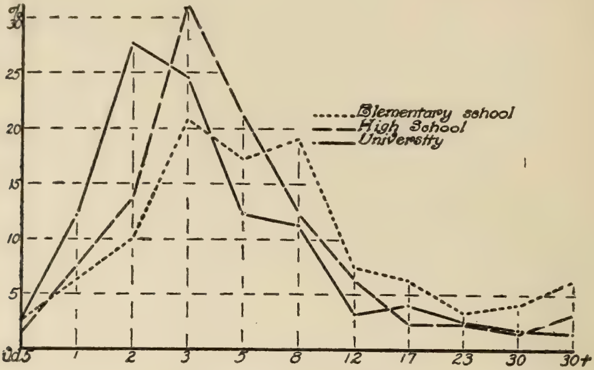


Fig. 14. Comparison of threshold of pitch discrimination for different ages.

TABLE X (Fig. 11, 12, 13 and 14). Variation with age and sex (Preliminary tests).

		Elementary School		High School		University	
	v.d.	*M*	*F*	*M*	*F*	*M*	*F*
Grade C	30+	8	5	3	3	0	0
	30	5	3	2	1	1	0
	23	5	2	3	2	2	1
	17	8	6	2	2	3	4
Grade B	12	8	6	6	5	3	5
	8	17	20	12	14	9	11
	5	17	19	21	20	13	11
Grade A	3	18	22	26	34	22	25
	2	8	10	14	13	26	32
	1	5	6	10	5	14	10
	½	0	1	1	1	7	1

M males; *F* females; numbers indicate the per cent. of cases at each step. A 4 v.d. or less; B between 4 v.d. and 14 v.d.; C above 14 v.d.

TABLE XI. Distribution by age and group in terms of per cent. of cases

Age	9	10	11	12	13	14	15	16	17	18	19+
Group A	30	24	24	16	17	12	13	12	8	4	8
Group B	38	44	40	44	41	44	40	34	34	47	26
Group C	32	32	36	40	42	44	47	54	58	49	66

The comparison of the mean variation for the three groups of ages given in Fig. 15 shows that the reliability of the records of

the high school pupils is practically as good as that of university students. Elementary pupils are slightly inferior but not so much as we would ordinarily find in other tests of discrimination.

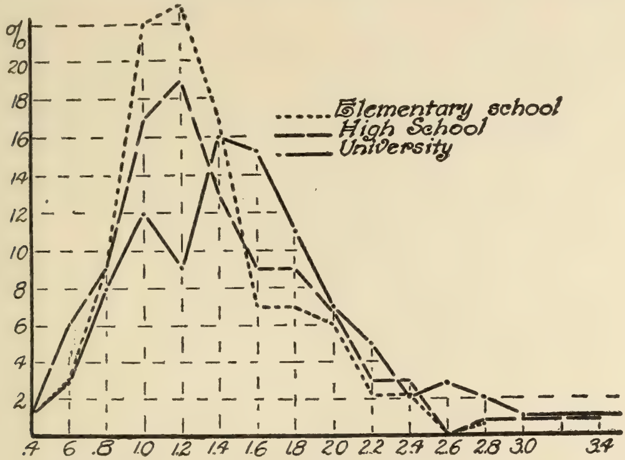


Fig. 15. Comparison of mean variation (m.v.) in the preliminary tests for different ages.

Some records of practice in pitch discrimination have been collected incidentally in this laboratory. The case of C.E.S. is presented (Fig. 16) to illustrate how variable the threshold may be aside from practice. The first practice series of twenty half-hour periods was taken in 1898 with crude methods. No resonator was used, the forks being held to the ear. This, perhaps, introduces the largest source of error in that series. Unfortunately data are not available for determining other causes of the inferiority of this record. Beginning with 1906 the Koenig resonators were used with a good quality of forks. The fact that, from this point on, the record is fairly constant would seem to indicate that the absence of the resonator in the foregoing series was the chief source of error. In 1907 the experimenter was not skilled. In 1910 the tests were made for the purpose of comparing certain conditions of environment, such as the light and sound-proof room, a class room, and out in the open air. From the 43rd to the 48th day a study was made of the effect of the duration of the tone and the time interval between the two tones. On the last four days distractions were introduced. The best record was made while the observer was intentionally tracing a maze.

Something was wrong in 1898. M. C. W. (Fig. 17) made a poor record in the twenty period practice undertaken by the same method and means as in the case of C. E. S. above. In 1908, as soon as the good resonator was introduced, her record was fine and free from fluctuations. She had, however, learned to play the violin and had gained experience in the tuning of forks in the years that elapsed since 1898. Her best records were made with distractions—tracing a maze or crocheting. These records furnish most striking evidence of the importance of reliable apparatus and technique.

In Fig. 18, characteristic results of practice, under most favorable conditions of control are shown; a, b, c, and d are the respective practice curves for four graduate students practicing one hour daily, sixteen days.

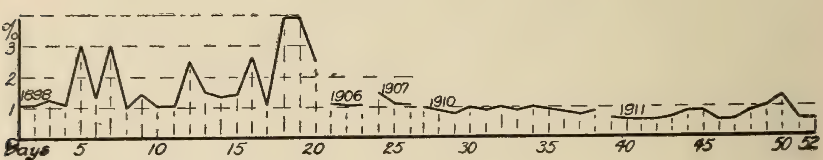


Fig. 16 Record of C. E. S.

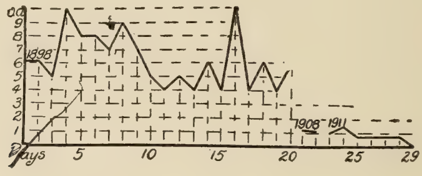


Fig. 17 Record of M. C. W.

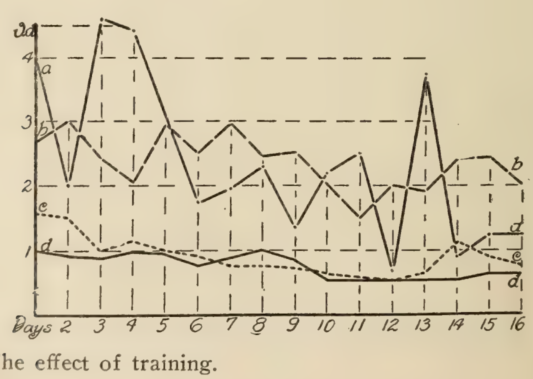


Fig. 18. The effect of training.

Factors in the development of pitch discrimination

Practice in pitch discrimination means (1) the control of a special set of cognitive factors involved in learning to recognize differences in pitch, and (2) the acquisition of skill in listening to musical tones. In most of the recent experiments on practice, such as those made by Book on typewriting, Swift on tossing balls, Bryan and Harter on telegraphy, Judd and others on handwriting, and Huey on reading, the object has been to determine the mode of acquisition of certain special habits. Of these Book distinguishes two sorts, habits of manipulation and habits of control: The latter he defines as certain general or more purely psychic habits acquired in the course of practice. It is to this type of learning that the present analysis is directed. Of these general habits or modes of control we may distinguish three types, (1) sensory control, by which is meant acquaintance with certain sensational facts, such as auditory qualities of the tones, and muscular sensations; (2) associational control, or acquaintance with memory images, as auditory, visual, and motor; and (3) control of special attitudes, as feeling of familiarity, most favorable form of attention, interest, etc.

Auditory and kinaesthetic sensations seem to play the leading rôle in judging differences in pitch. Two types of observers may be distinguished. First, there are those whose perception of pitch is chiefly in terms of tonal qualities. They learn to direct attention to the characteristic sharpness or fineness of the high tones and to the flatness or dullness of the low tones. The particular sensory quality of the tone varies with different persons. One notices that the high tone is sharper, and has a tendency to last longer in the ear than the low tone. Another describes the high tone as finer and more piercing. The lower tone is usually distinguished from the higher as being duller, deeper, heavier, and more mellow. It is also interesting to note that some observers judge altogether by the high tone, while others judge only by the low tone. Some persons seem to have an affective preference for high tones, others for low tones. This forms an apperceptive basis for the judgment.

Second, there is a considerable number who depend largely upon kinaesthetic sensations of the vocal organs in making the judgment. Regarding these Stumpf says: "If the muscular sense in the vocal organs is the same as a former tone that we have heard, we judge that it is the same tone. If we are told that a certain

tone is A, we remember that a tone giving the same sensations is A. If the muscular sense changes in a definite way when we sing two tones, we say that the tones rise. If a distance is noticeable in the change, we judge the second interval to be greater." Stricker did not think of music in terms of notes nor of auditory images, but in terms of muscular sensations in the vocal cords. He speaks of the impossibility of the reproduction of a tone in the memory without bringing into play the actual or intended use of the vocal organs. He considers the connection between tone perception and the innervation of the vocal organs a sort of reflex.

This view is in accord with many introspections in the present investigation. Some of the observers allege that they are not able to tell whether the second tone is higher or lower until they reproduce the tones either audibly or mentally in terms of vocal strain. In one individual test the observer, a university student, was not able to distinguish a smaller difference than 20 v.d. by merely listening to the tones. When he was told to hum the tones, he immediately ran down to 8 v.d. and continued to improve, reaching 2 v.d. Singing seems to enhance the power of discrimination partly on account of the timbre of the voice and partly on account of the motor elements in vocalization. "I carry the first tone over and when I hear the second I hum it to see whether I feel more or less strain in the vocal cords."

These muscular and kinaesthetic sensations are not always confined to the vocal cords. They may start in the vocal cords and spread to other organs; as, for example, "A strain starts in the vocal cords and runs up through my head." The sensations may be initiated in other organs, or they may be felt as general bodily changes. "I feel the tone as a singing in my head." "In case of the high tone, the singing is 'stronger' than in the case of the low." "The high tone seems to make a stronger impression in my ears than the low tone." By impression the observer probably means muscular strain. "I feel the tones as vibrations in the body. They seem to go all through me and cause a sort of strain." "I have a tendency to breathe more deeply for the low tones." "The high tones give me a sense of elation; I seem to mount. The low tones seem to give me an experience of gentle relaxation, a general feeling of calm." "I have a distinct tendency to move up and down according as the second tone is high or low." "Low tones seem to

drag me down; high tones seem to lift me up." "I feel an upward impulse and tend to rise with the high tone." "The mind seems to be a little more tense for high than for low tones." The affective quality of the tone is often the important element in consciousness. "When the low tone follows the high tone it seems to be more pleasing." "The high tones feel different but I can not explain the difference."

In most instances the auditory and kinaesthetic sensations combine into an auditory-vocal perception. Thus the judgment is a highly complex process conditioned by a mass of auditory and muscular sensations. The total result in consciousness, however, is a simple experience, a mark of familiarity which enables the observer to interpret the difference immediately.

In addition to sensory processes the judgment is conditioned by certain associational processes, chiefly auditory and visual images together with certain associations which are built up around these.

Many speak of carrying over the memory of the first tone and comparing the second tone with it through auditory imagery. The clearness of the image, and hence the certainty of the judgment, depends upon whether the interval is short or long. This varies somewhat with different individuals as does also the character of the imagery. Some observers associate certain familiar tones with the image of the present tone. The low tone sounds like the "hoot of an owl." The high tone is associated with the major key and the low with the minor key, or perhaps the observer imagines he hears his own voice singing the tones.

Visual imagery includes localization in space, voluminousness, and color-tone qualities. "The high tone seems to glide up at the end." "High tones seem nearer and low tones farther away." "I have a visual image of a teeter board." "High tones seem to be in the upper part of my head; low tones in the lower part." "I think of ti, do, or do, ti in the musical scale." This observer was unable to describe the tones in terms of auditory imagery. "The high tone appears to be higher up in space than the low." This reply is typical of a large number and seems to play an important rôle in the perception of difference. "The high tone has a swelling, expansive feel in the left ear and seems to have a pull upwards,—a lifting quality—almost to the point of unpleasantness in strength. The lower tone seems to be located in the right of the direction of

the head and below. The high tone is nearer the head, the low tone far away."

In the following case the method of localization is unique. The relative position of the two tones is the reverse of what is usually found. "The low tone appears to be above the high in space. It is also larger so that the two tones would be represented by a heavy above a light line, thus: "

Many observers refer the tones to a musical scale or musical instrument. M. O. thinks how she would play the violin to produce the different tones. H. S. sees her finger move up and down the violin string. O. S. says, "When I think of the second tone as higher I think of it as higher up on the piano." Another says, "I seem to see my fingers moving along the violin string."

M. C. W., a trained psychologist, locates the tone by a peculiar kinaesthetic-visual imagery. The high tones go up to the right and lie in the head, the low tones move down to the left and lie near the left side of the root of the tongue. The first sound is in the aural axis, a little to the right. All are thought of as in the head, though she knows the real source.

"High tones seem long and pointed while low tones seem big and flat." M. describes the interval as a pyramid or cone with the high tone at the apex and the low at the base. For W. "High tones are fine and sharp. They seem thin and compact: I imagine an object contracting. Low tones are relatively rich."

Colored hearing plays an important part in the judgment of some observers. Moritz Katz[4] has reported on color impressions of Schumann, Tieck, Liszt and others. In the present study the following are noted: "The high note seems to be a brighter color, the low darker." "High tones are bright and clear; low tones are dark and murky." M. always thinks of sounds in terms of color. Her impressions are remarkably complex and varied. "When I hear sounds that please me they appear violet. When I am talking with any one whose voice is pleasing, I see violet color. When I listen to a soprano solo I see a section of the rainbow. As the tones become higher they change to bright green and the very highest tones appear like little flames of fire. Low tones are reddish brown. Any rasping or disagreeable sound appears red or brown. When I hear a chorus of mixed voices or an orchestra there seems to be a large mass of violet color and from this on all sides little short

[4] Zeitschr. f. angew. Psychol., 1911, pp. 1-53.

tongues of various shades of green, yellow, and red." She does not remember when she did not translate sounds in this way. L. M., 17 years old, combines spacial and brightness qualities with tones. A very high tone appears to be a bright vertical line. As the pitch is lowered the line grows in width, but diminishes in brightness.

As regards the most favorable form of attention we have two factors, the direction of attention and the level of attention. As regards the direction of attention three modes are possible. One may attend to the beginning, the middle or the end of the tone. More than half of all observers select the middle of the tone as the critical point. The rest are about equally divided between the beginning and end of the tone. Closely connected with these modes of reaction is the snap judgment. With the organism set for a definite point in the tone, the judgment is made the instant this point reaches the focus of consciousness. This form of judgment when once brought under control almost always favors improvement.

It is also observed that it is easier to judge which tone is higher or lower if the forks are presented abruptly. If the tone swells gradually from a faint beginning, it appears to raise the pitch slightly and thus confuses the judgment.

The most favorable level of attention varies with different individuals. The introspections show that for some the closest attention to the tones is required for successful work. Others say the very keenest attention causes high nervous strain which leads to mistakes. The writer has observed this very definitely in his own case. T. F. V. says, "Much depends upon my attitude. If I hold myself in a passive attitude and answer with ease, in a reflex way, I am quite sure to be correct in my judgment; but if I get the attitude of strict attention I cannot do so well. If I can keep in a state of relaxation, I experience no difficulty in giving the judgments." Practice usually results in what Professor Welton calls receptive recognition. When one becomes familiar with tones there ceases to be that active attitude of attention which characterizes the first few tests. The two tones are not thought of separately, but the interval is grasped as a whole and is interpreted by its total effect in consciousness. The factors which enter into the judgment do not come into consciousness, but remain unconscious. All that the observer can state is that he knows the instant he hears the second tone whether it is high or low. There is no consciousness of a memory image and no comparison.

To determine the effect of distraction three series of tests were made. In the first the eyes were closed (no visual or motor distraction); in the second the eyes were kept open and allowed free movement (normal distraction); and in the third the observer was required to trace a maze while performing the test (regulated distraction).

The effect seems to be about equally distributed between helping and hindering. All but two were appreciably aided by distraction at the beginning of the series. At the close only two were especially aided and only one found distraction a hindrance. In all the other cases, when the distraction method became automatic, it ceased to influence the results. Moderate distraction seems to be an aid chiefly as a means of raising the level of non-voluntary attention. The best form of attention for a majority of observers seems to be a periodic fluctuation between sharp and instant attention to the tones and complete diversion during the interval between two pairs of tones. The problem of distraction is an exceedingly complicated one. Perhaps the most striking result of this series of tests was the demonstration that distraction enters even when we are most expected to concentrate upon a single task. Table XII shows the per cent. of right judgments with and without distraction.

TABLE XII. The effect of distraction

Observer	With Distraction	Without Distraction
1	93.9%	92.3%
2	86.2	84.5
3	89.0	91.3
4	91.2	87.4
5	90.8	87.5
6	88.7	92.6
7	86.8	91.5
8	95.8	96.3
9	92.5	91.5
10	93.3	97.3

The following extracts from introspections illustrate the general mental attitude toward the different methods. These were written at the close of the series of tests and express the observer's impression at the time.

D. A. A. "In the beginning of the test I felt distinctly dissatisfied with surroundings and was annoyed by the peculiar effect the room had on the quality of the tone. This was overcome in about twenty minutes. The maze failed to arouse interest and hence was of no assistance—quite the contrary, it was really an annoyance—a distraction unfavorable. During the time I took 100 with eyes closed I was able to inhibit any absorbing interest in anything except the discrimination of tones. I was able to concentrate

very definitely on the work in hand. Following immediately with closed eyes on a test with 0.5 v.d. I grew tired of the uniform method (eyes closed) making two mistakes in the first 50 and six in the second 50. About five of the errors were attended by a feeling of decided uncertainty and the others were caused by some annoyance. Returning to the use of the maze for the last 100, 0.5 v.d. I voluntarily renewed interest in the affair and raised my record making but four errors in the 100 judgments."

J. E. B. "At first the maze troubled me, but after going over it a number of times I could do it rather automatically so that more and more attention was given to the tones. When the eyes were open there were always numerous disturbances that would effectively distract attention. With the eyes closed there was very little to distract attention."

M. C. "Working at too great tension seemed to be my greatest difficulty during the tests, especially with eyes closed and eyes opened. The maze seems to relieve that tension though I rather expected the opposite effect. As a result of the tension I found myself confused at times and made several errors in succession. I could not notice much difference in my own attitude toward the test with eyes closed and that with eyes open. Possibly there was a greater effort to center attention on the two sounds when my eyes were open. In both of these tests there were times when I seemed to notice a difference in pitch, but could not tell which sound was higher. This difficulty seemed to disappear with the maze test."

N. E. G. "I felt that it was much easier to decide with my eyes closed than with the maze. However, my third record shows fewer errors with the maze."

P. H. H. "In the test using the maze it seemed easier to concentrate the mind; that is, the mind was concerned with two definite things: the maze and the tones, as opposed to the free associations. There was less inclination to drift to other things. The decisions in the maze test involved less conscious effort and seem to be 'felt' rather than consciously formed. Errors in the maze test often followed the effort to locate the end of the pencil line, after it was lost through the recording of the introspection. I gained better success by starting the maze line ahead of the trial."

T. F. V. "I found in this experiment that everything depended upon my attitude towards it. If I had my attention in high strain to perceive the difference between the tones and to give a correct judgment my results would be very poor. However, if I fixed my attention upon something else and gave almost passive and indifferent attention to the forks my judgments were far more certain. When my eyes were closed I attempted to focus my attention upon the retinal light and also attempted to complicate matters by means of eye-movement. That is to say, I was endeavoring to center my attention on something other than the forks. In the maze, the more intensely I worked with respect to accuracy and speed, the more clear seemed the distinction between the forks. This focusing of my attention on something else than the thing in hand was very hard to do, especially after I made one or two mistakes in close succession. If I had not been so desirous of getting correct judgments I am sure my discriminative ability would have been better."

L. E. W. "My preference is for the maze, eyes open, next and last of all eyes closed. In the latter case my mind is ever full of visual and auditory imagery, rich and prolific. One moment I am in my room and can hear the clicking of my typewriter, the next I am singing some haunting air, then I see a paper on my table I should have brought with me this morning. Sometimes I recall in auditory imagery just what the order of the last two forks was and I feel sure that I was wrong though I had unconsciously made the wrong reply. The main difficulty with keeping the eyes closed is that in so doing I can't keep a constant image or position before me; my mind refuses to remain a blank. Now, with eyes open I can fixate my eyes on some particular object and as long as this does not waver and my thoughts and attention are on the business at hand I feel secure—am so, in fact. With the maze I direct my attention to one thing continuously."

E. D. S. "Yesterday I was interested in the maze and hence was distracted by it. To-day I felt no such interest in the maze. In the test with eyes closed I became interested in the method of presenting the forks and was thinking about certain possibilities of modifying the method. This became a distraction or rather a constant object of attention and source of error."

The second function of training in pitch discrimination is the acquisition of skill in listening to musical tones. Four factors are involved. First, skill means raising the level of non-voluntary attention. The power to concentrate upon the characteristic acuteness or gravity of the tones without conscious effort usually favors correct judging and is the end to be sought in ear training. Swift found that strained attention results in distraction, and a number of observers make similar statements regarding their own experience in distinguishing tones.

Second, skill means mechanizing the conscious factors in learning to distinguish differences in the pitch of tones. The pupil has learned to image the tones as auditory, auditory-vocal, kinaesthetic, or motor qualities. In this process some one or two qualities have predominated, and the object of ear training is to form habits of listening to, *i.e.,* of thinking musical tones in terms of their dominating imagery.

The third factor in the acquisition of skill is interest. One of the chief aims of ear training should be to enlist the pupil's interest in the appreciation of musical tones and the enlargement of the scope of apperception with reference to isolated tones.

The physiological limit

The physiological limit is undoubtedly considerably lower than is indicated by the threshold which would give 75 per cent. right

cases, as here used. To demonstrate this and, at the same time, to observe the significance of the choice of a particular increment in the homogeneous method, measurements were made on seven good observers whose threshold had been recorded as being in the neighborhood of 1 v.d. Four tests were made on each of the seven observers at 1, 0.5, and 0.25 v.d. with 200 judgments at each unit in double fatigue order, or a total of 800 judgments at each unit. From the per cent. of right judgments the probable threshold with 75 per cent. right cases was computed by the Fullerton-Cattell formula.

Table XIII shows the difference threshold which was required to give 75 per cent. of right judgments for 1, 0.5, and 0.25 v.d. respectively.

TABLE XIII

No.	1 v.d. v.d.	0.5 v.d. v.d.	0.25 v.d. v.d.
1	.42	.44	.49
2	.40	.27	.23
3	1.30	3.33	1.30
4	1.10	1.47	1.10
5	1.75	1.47	1.30
Average	.82	.74	.60
6	1.00	1.00	
7	1.54	1.30	
Average	1.24	1.17	

The first five observers had more than fifty per cent. of right judgments at 0.25 v.d.; hence the threshold is calculated for the three increments of the other cases. Number 6 got only 49 and number 7 only 46 per cent. right cases on the 0.25 increment. But the significant fact is that for both of these persons the number of right judgments on 0.5 v.d. was such as to give practically the same threshold as was found on 1.0 v.d. Only one important inconsistency occurs in the above table. In the case of No. 3. the right judgments at 0.5 give a threshold of 3.33 v.d. while at 1 v.d. and 0.25 v.d. the threshold is exactly the same.

Examination of the table therefore proves that in the region of the average physiological limit the conventional threshold may be computed on the basis of observations considerably below that limit (here in five cases out of seven) and that the actual physiological limit is always considerably below the conventional threshold. This is, of course, analogous to what we find in sight; under exception-

ally favorable circumstances we may see a small, well defined object at a distance which, from the nature of the dioptric system, represents the physiological limit of acuity in vision but average records of acuity would ordinarily designate a point short of that distance.

Correlations

From the standpoint of musical training it is important to know how the ability to distinguish differences of pitch is correlated with other mental characters, as general intelligence and singing ability. In addition to these we wish to know whether brothers and sisters are more closely correlated in ability to distinguish differences of pitch than other children not related. These questions are discussed in their order.

For the purpose of the correlation between pitch discrimination and general intelligence and singing ability, the data for pitch discrimination were obtained from the final days of the practice series. No single absolute measure of general intelligence is possible. For the present purpose the teachers were instructed to mark general intelligence on the basis of two criteria, brightness and reliability, assuming these to be of equal weight. By brightness is meant quickness and accuracy of mental grasp, or, in other words, general wide-awakeness. Reliability is self-explanatory. It is the correlate of a small mean variation for daily work. For convenience of marking, these two factors may be considered as having equal weight and may, therefore, be marked independently on a scale of 10. It was explained that the markings should follow approximately the normal distribution for each age and for both sexes. The mean of the two marks was taken as the mark representative of intelligence.

In order to facilitate correlation the ten units in the series of increments used in pitch discrimination were translated into corresponding values on the scale of 10, thus: 30 v.d. corresponds to 1; 23 to 2; 17 to 3; etc. .... 0.5 v.d. to 10.

The markings on singing ability were also based on the teacher's judgment of the pupil's ability to sing correctly in pitch scale and a melody.

As regards kinship, three correlations were as follows: (1) between younger and older brothers and sisters with practice; (2) the same without practice; and (3) between the younger members

of the second correlation and other children of the same age and sex as the second members, but not related.

The correlations were determined by the Pearson product-moments method. In order to show the relative distribution of individuals for each factor correlated, each group is subdivided into five grades. This is not a quintile subdivision as there is no attempt to have an equal number of persons in each subdivision. The distribution by grades serves the purpose of comparison quite as well as the quintile or quartile method and avoids the necessity of ranking, which is practically impossible on a scale of 10 units. The method of subdivision is very simple. The scale of 10 units is is divided into five equal parts. 1 and 2 = E. 3 and 4 = D. 5 and 6 = C. 7 and 8 = B. 9 and 10 = A. An example will make clear the method. An observer gets 3 in pitch discrimination and 7 in general intelligence. He belongs to Grade D in the first factor and in Grade B in the second factor. The number who are in the same grade in each factor indicates the degree of correlation. The number who are in different grades in the two factors indicates lack of correlation or low correlation.

The results show a relatively high coefficient of correlation between pitch discrimination and general intelligence, singing ability and musical training (Tables XIV and XV). It is higher for boys than for girls and highest for both boys and girls between pitch discrimination and general intelligence.

TABLE XIV. Correlation of pitch discrimination with general intelligence and singing ability

Pitch discrimination with

(1). General intelligence	Boys	r	.70	p.e.	.023
	Girls	r	.63	p.e.	.026
(2) Singing ability	Boys	r	.71	p.e.	.023
	Girls	r	.51	p.e.	.031

TABLE XV. A. Pitch discrimination and general intelligence

Boys — Intelligence

	234	A	B	C	D	E
	A	4	15	16	3	
	B	6	31	29	3	1
Pitch	C	3	25	33	11	
	D	1	8	19	8	
	E	1	4	4	9	

Girls — Intelligence

	274	A	B	C	D	E
	A	6	14	14		
	B	12	51	27	3	
Pitch	C	11	49	37	12	
	D	4	3	17	4	
	E		1	6	3	

B. Pitch discrimination and singing ability

Boys

		Singing				
	234	A	B	C	D	E
	A	5	7	3	2	
	B	17	22	13	8	4
Pitch	C	4	24	23	12	7
	D	2	13	16	8	6
	E	1	4	15	8	11

Girls

		Singing				
	274	A	B	C	D	E
	A	11	21	12	1	
	B	8	46	38	10	2
Pitch	C	8	27	39	16	
	D	3	8	16	2	3
	E					

The fact of a high correlation between pitch discrimination and general intelligence favors the conclusion reached above that pitch discrimination depends partly upon the ability to learn, *i.e,* upon brightness and reliability. If this is a correct view, training in pitch discrimination is essentially mental training. It is more than reproducing tones; it is thinking tones. Another conclusion which is in harmony with what has just been said is that a child may possess a perfect ear for tones, and still be unable to distinguish differences in pitch. Musical training should begin with training in tone quality.

The coefficient of correlation between pitch discrimination and singing ability is technically high. A high correlation between these factors means that the ability to distinguish differences in the pitch of tones is an essential factor in learning to sing.

Table XVI shows that, for the groups compared, girls are superior to boys in pitch discrimination, since there are no girls in Grade E and relatively few in Grade D. But they are not shown to be essentially superior in singing ability.

TABLE XVI. Correlations for blood relationship

Correlation between pitch discrimination of				
(1). Brothers and sisters:				
(a) with practice	r	.48	p.e.	.031
(b) without practice	r	.43	p.e.	.035
(2). Children not related	r	.53	p.e.	.030

The coefficient of correlation between brothers and sisters on the basis of ability in pitch discrimination is not higher than between other children. This is true both for records without practice and records after practice. Although the results are clearly negative, no sweeping conclusion should be drawn because several variables are involved, such as advantage of the knowledge which comes with age, differences in intelligence, the element of competition, etc. This is regrettable since it had been definitely hoped and planned

that this large collection of data might contribute to the solution of this interesting question. Finally, the younger member of each pair in the second correlation was compared with another child of the same age and sex as the second member, but not related. The coefficient of correlation is practically the same for the three groups. (Table XVII). No conclusions can be drawn from these meager results as regards the influence of heredity on tonal hearing.

TABLE XVII. Correlation of pitch discrimination for younger and older brothers and sisters

(1) With practice

		Older				
	129	A	B	C	D	E
	A	7	5	1		1
	B	9	32	12	5	1
Younger	C	4	17	7	4	2
	D		9	2	3	
	E		3	2	2	1

(2) Without practice

		Older				
	275	A	B	C	D	E
	A	2	13	12	7	1
	B	2	6	17	12	1
Younger	C	12	12	54	27	10
	D	7	8	25	32	5
	E	1	1	2	4	2

(3) Children not related and without practice

		Older				
	275	A	B	C	D	E
	A		4	2		3
	B	6	22	28	12	8
Younger	C	6	38	36	15	9
	D	2	15	23	4	2
	E		13	16	4	7

General conclusions

The quantitative statement and analysis of data has been presented in such condensed form that a summary of conclusions from that point of view is scarcely necessary. There is, however, need of a statement of "general conclusions" from the point of view of interpretation and application of the experimental results in the light of the quantitative data, the introspections of the observer, the daily notes of the experimenter, and a general study of the problem with the collaborators in research. Such a statement necessarily involves something of a personal equation and I am glad to acknowledge in this the co-operation of Professor Seashore whose long and varied experience in this field of research makes this interpretation possible.

The psychological limit in pitch discrimination is always below the conventional threshold (75 per cent. right cases). Thus, a person whose threshold is 1 v.d. may, under extraordinarily favorable circumstances, hear as small a difference as .25 v.d.; and it is probable that in the normal unreflective and uncritical appreciation of music the automatic "impression" of tone differences comes freely through this region of increments which are below the conventional threshold. This conventional threshold which can not be further reduced by instruction or training we have called the "approximate" physiological threshold. This is the concept of threshold that must be employed for most purposes of research and in nearly all applications of the test for practical purposes. The three factors which differentiate it from the true physiological threshold are—the convention of counting 75 per cent. right cases, the physical variation in the organ of Corti, and the failure to keep all the conditions of the measurements under control.

Success in making a true measurement on an unexperienced observer in a single sitting varies with the knowledge, keenness, and care of the observer and the many objectively favorable or unfavorable conditions of the test as well as the experimenter; but, everything taken into account, it is safe to say that when an individual test is made under favorable conditions the approximate physiological threshold may be reached in a single sitting of less than an hour for more than half of the cases of adults or children who are bright and old enough to understand the test. Even in group tests by the heterogeneous method one may reach in an hour the approximate physiological threshold of nearly half of the observers who are old enough and bright enough to observe.

A cognitive threshold, always above the approximate physiological threshold, may be due to failure in understanding what is required in the test, lack of information, defect in auditory imagery and memory, lack of application, confusions, objective or subjective disturbances, expectations, inhibitions in writing or speaking, etc. Most of these conditions are such that they may be removed by information, by inducement to use the best effort, or by learning through some experience.

There are means of determining when the approximate physiological threshold has been reached. Chief among these are the mean variation and the character of the distribution of the errors. But in

individual tests many direct observations on the character of the difficulties in judging may be helpful. In general, where a record is low (good) the chances are that the observer has no "cognitive" difficulties. The uncertainty is, of course, always with reference to the poor record. Practical advice or recommendation should therefore be cautious in the case of poor records for fear that the limit reached, although persistent, may be merely cognitive. One can not err on the side of getting too good a record; the danger is always that something has prevented a fair test of actual ability.

The sensitiveness of the ear to pitch difference can not be improved appreciably by practice. There is no evidence of any improvement in sensitiveness to pitch as a result of practice. When a person shows a cognitive threshold practice ordinarly results in a clearing up of the difficulties which in the way of a true measure of discrimination by information, observations, and the development of interest, isolation of the problem in hand, and more consistent application to the task in hand. This is, of course, not improvement in the psycho-physic ear but merely a preliminary to a fair determination of the psycho-physic limit. It follows that instruction in regard to the nature of the test and individual help are all important for the lowering of the cognitive limit and that mere practice for this purpose is a poor and uncertain makeshift. It also follows that a "cognitive" threshold is no measure at all but rather a confession that the measurement has not yet been successfully made.

Training in pitch discrimination is not like the acquisition of skill, as in learning to read or to hear overtones. It is in the last analysis informational and the improvement is immediate in proportion to the effectiveness of the instruction or the ingenuity of the observer and the experimenter in isolating the difficulty.

Reduced to its lowest terms the question of variation with age may be interpreted to mean that we have no evidence of improvement in the psychological limit of pitch discrimination with age; a young child of school age and even younger, can hear pitch fully as keenly as an adult. The amount in favor of the adult shown in all group statistics is amply accounted for by the difficulty in making a reliable test on the young and by their lack of information. This statement is based primarily on two lines of evidence,—the common occurrence of fine, irreducible records among young children, and the character of the conditions which are ordinarily overcome by instruction and training.

Pitch discrimination does not vary with sex to any significant extent. In the records here reported and in the many hundreds of other records in this laboratory in which comparisons may be made for sex, certain tendencies are shown in groups of records, sometimes in favor of one sex and other times in favor of the other sex, but on the whole, it seems certain that such differences, except so far as they are due to grouping, may be accounted for as due to the conditions of the test rather than to the sex difference in the psycho-physic capacity of pitch discrimination. Thus one of the most consistent and striking differences reported above, that of the superiority of elementary schoolgirls over elementary schoolboys may probably be fully accounted for by the prevailing trait of aloofness of the preadolescent boy toward music. These boys often regard music as a sort of frill for girls and, therefore, enter the test with less fervor than do the girls. Such interpretation is supported in part by the fact that in the high school and in the university, where the girls have had far more advantage of training than the boys, the records reveal no appreciable difference for sex.

Not a single case of tonal deafness was isolated in any of the records here reported. This would indicate that if tonal deafness exists at all in a "normal" ear, it is no so common as has usually been supposed.

We have found a high correlation between pitch discrimination and ability in singing, as judged by teachers. In the collective records there is also a high correlation between pitch discrimination and "general intelligence." This is undoubtedly due to the presence of so many "cognitive" as opposed to physiological thresholds.

Under the conditions of this test the records of members of the same family do not correlate more closely than do members of different families.

This test is elemental, *i.e.,* when applied under favorable conditions it calls forth a relatively simple and immediate sensory discrimination which does not improve appreciably with practice. It is like the minimum visible angle in visual space—the limit is set by the sense organ. We say "under favorable conditions" because the cognitive factors which condition a fair test must be recognized. As has been seen in a large per cent. of cases, we can get only a cognitive threshold in the first attempts. As elemental, this test is contrasted with, *e.g.,* a test of ability to isolate overtones

in a violin tone which represents a skill that can only be acquired through practice. It must be recognized that the test is a true and successful test, the results of which may be applied with safety, only as it is actually elemental.

The basal character of pitch discrimination in the appreciation and expression of music has become evident in many ways. Keen recognition of pitch difference is a condition of auditory imagery, auditory memory, singing or playing in true pitch. This is true as well for the affective attitudes with reference both to pitch and to timbre, for timbre is in the last analysis simply a pitch complex. It would therefore seem to be most fundamental of all tests of musical talent, although, of course, no one test by itself can be considered an adequate measure of such talent.

The educational value of this test has been strongly impressed during this work. It is unquestionably the isolation and measuring of one specific, basal factor in musical talent. It may be undertaken individually or in groups and commends itself particularly as one of the tests that should be made in schools for the purpose of vocational guidance in music, in the music studio for the purpose of learning where to place the emphasis in instruction and in adapting the course to the natural capacities of the student, and as a recurrent exercise in the schools and in the studio for the purpose of developing keenness in attention to detail of tone in ear training.

The instruments, *i.e.,* the tuning forks and resonators as here used, and the method, both the heterogeneous and the homogeneous procedure, have proved eminently satisfactory.

THE LOWER LIMIT OF TONALITY

BY

THOMAS FRANKLIN VANCE

An accurate determination of the threshold of the lowest audible tone involves a consideration of the variables which condition it. The area and the amplitude of the wave and the distance of the vibrating body from the ear of the observer are the principal objective variables. Individual differences, due largely to innate capacity, degree of practice, and ability to concentrate attention, and variations within the same individual which may be attributed to changes in physical tone and mental content, are obviously the most influential subjective variables. These variables, both objective and subjective, present particular problems which must be considered in their relation to the general problem of the lower tonal limit before the latter can be accurately determined.

No attempt will be made here to review the history of investigation on this problem. A good summary is found in Titchener's Instructor's Manual, Quantitative.

Mr. Misao Imai made a careful study of this problem in this laboratory in 1907. Inasmuch as his results have not been published and the present study is essentially a repetition of his work for the purpose of verification it is necessary to report his work in brief.

Mr. Imai's first problem was to determine the relation between the threshold and the amplitude of the wave. He produced the tones by an electro-magnetic fork 460 mm. in length and 10 mm. by 20 mm. in cross section of a prong. By differential weights five tones could be produced, namely, 35, 25, 22, 19, and 17 v.d. By varying amount of resistance different amplitudes could be secured. The test in each case consisted in determining the smallest amplitude that would produce an audible tone at a given pitch. The measurements were made on ten laboratory students. With this apparatus, he obtained the results shown in Table I.

TABLE I. The relation of threshold to amplitude

v.d.	ampl. in mm.	m.v.
30	1.30	.15
25	1.75	.30
22	2.20	.45
19	2.95	.50
17	3.45	.50

From these results he drew the conclusion that the threshold varies inversely as the amplitude; *i.e.,* increase of amplitude lowers the threshold.

With the same apparatus he conducted a second series of experiments to learn the relation between the distance from the ear of the vibrating body and the threshold. From these results he concluded that the distance at which the fundamental tone is just perceived, varies with the pitch; *i.e.,* the higher the pitch, the greater the distance may be, within given limits. Below 18 v.d., however, he found the distance uncertain as the overtones were frequently confused with the fundamental.

It then occurred to him that the area of the vibrating body might have an important bearing upon his general problem. He varied the area by means of four pairs of discs 6 mm. in thickness with diameters of 6, 8, 10, and 12 cm., respectively, which could be attached to the ends of the prongs of a fork similar to the one described above. With the variable of area thus controlled, he learned that it must always be given due consideration in the determination of the lower limit of tonal hearing. Judgments from ten highly practiced observers showed clearly that the threshold varies inversely as the area, within limits.

Investigators previous to Imai had used three different methods in the production of tone; namely, (1) vibrations of tuning forks, pipes, and reeds; (2) difference tones; and (3) interruption tones. Helmholtz, Stumpf, and Preyer favor the use of tuning forks. Where forks have been used the thresholds are, as a rule, noticeably lower than where other means have been employed. Schaefer views with suspicion all thresholds reported under 16 v.d., inclining to the belief that perceptions below that point are conditioned by overtones rather than by fundamentals. He doubts von Bezold's assertion that the fork by means of which he registered a threshold of 11 v.d. was free from overtones. Schaefer is doubtless in the right in his contention that von Bezold has not proved this point conclusively. Von Bezold's statement that the very low tone 11 v.d. was perceived by some observers with normal hearing, cannot be accepted unqualifiedly. In fact, the statement would have been more convincing had he admitted the probability of overtones. Unless an observer realizes the possibility of an overtone and is cautioned to discriminate between it and the fundamental, he will base his judg-

ment upon the first tone perceived which below 18 v.d. will always be an overtone, if the tone is not pure. Preyer reported the very conservative threshold of 18.6 v.d. Wundt's threshold of 14 v.d. with an Appunn reed and 16 v.d. with a wire fork undoubtedly held true with good observers. They compare very favorably with the results of Imai who found the threshold to be about 16 v.d. with the plain forks.

Imai, then, by an increase in area obtained a lower threshold than those recorded by the most reliable previous investigators. His observers who had a threshold of 16 v.d. with the plain fork were enabled to hear 12 v.d. with the most favorable size of disc-forks. This lowering of the conventional threshold of tonality by about 4 v.d. was of such importance as to justify a verification and extension of his experiment under most accurate conditions of control.

In the investigation here reported tuning forks were used for the production of the tone because they are among the best means for producing relatively pure tones, and there seemed to be no other way to change the area of the vibrating surface without at the same time altering the rate of vibration.

Two electro-magnetic forks were arranged tandem. The driver fork was constructed of soft steel and measured 65.5 cm. in length from the base to the tip of the prongs which were 1 cm. by 2 cm. in cross section. A series of eleven holes, 1 cm. in diameter and 2.8 cm. apart, bored in each prong made the fork lighter and more active. The fork hung vertically from a support by means of a large iron hook securely bolted to its base. The driven fork was made of soft steel rod and measured 57 cm. in length. The prongs were 1 cm. in diameter. By means of differential weights, vibration rates ranging from 18 to 12 v.d. could easily be secured. Three pairs of discs of fibre 6 mm. in thickness and 2.5 cm., 5 cm., and 10 cm. in diameter respectively, served a double purpose; they not only afforded the desired variation in area, but also, served as weights for the tuning of the fork. The two forks were tuned to the same pitch. The driving fork was kept at a distance, thus causing the driven fork in the observing room to vibrate without the disturbance of an interruption spark and sound. The amplitude of vibration was varied by means of variation in the strength of the energizing current through an adjustable resistance coil in the experimenting room.

All the experiments were conducted in the sound-proof room. Beginning with 18 v.d., the series followed a descending order until the threshold was reached. Each rate, however, was observed with the three different areas of vibrating surface as controlled by the discs of 10, 5, and 2.5 cm. in diameter. The observations were completed at each step in the order named before the forks were adjusted to the lower rate. The observer sat in a comfortable position with one hand on the switch which controlled the current through the driven fork, and waited attentively for the tone. The experimenter, after a warning signal, presented the fork with the central part of the disc exactly opposite the opening of the more sensitive of the two ears and as close to it as the amplitude of vibration would permit. After a very brief period of observation, with the current either on or off as desired, the observer immediately reported "tone" or "no tone". The observer's judgment thus determined the amplitude of the fork in the next presentation. When very near the threshold of intensity the experimenter recorded the amplitude of vibration upon smoked paper. This method was followed until the least intensity of tone which the observer could possibly hear, for a given rate and area, was reached. Each period of observation lasted one hour; during that time data for each disc at two different vibration rates could usually be obtained. Three complete series from 18 v.d. to the threshold with the three different discs were secured for each observer.

The nature of the experiment demanded that only experienced observers be employed. Ten members of a class in experimental psychology were available. Each of these submitted to an eight hour course of training which was divided into periods of two hours each. From this group of ten, the experimenter chose three whose work in the preliminaries was most satisfactory to act as observers throughout the entire series. The demonstrator of this class and the experimenter brought the total number of observers up to five, all of whom had had considerable training in psychological experimentation.

Before turning to the consideration of the results, a brief statement of the difficulties encountered in a study of this kind should be made. That there are difficulties may be inferred from the fact that different investigators have placed the threshold at points ranging from 8 to 30 v.d. Low tones are intrinsically weak and

require much more energy for their production than do the high tones which are relatively strong and clear. While little effort is required in sensing a tone in the normal range, it becomes a matter of strict attention to perceive the tones near the limen where the predominant overtone may be confusing. It is not easy to procure a perfectly pure tone in the central rigister where the instrument used is small, but it is next to impossible to produce an absolutely pure tone with the large forks of the lower limit. Furthermore, we are not accustomed to pay attention to liminal tones, therefore, when a person is asked to observe them under laboratory conditions, he is at first at a loss to know what to listen for. It takes hours of training before the individual feels the necessary degree of certainty in giving his judgments. But patience and practice tend to make him reasonably certain of his judgments. The development in accuracy of observations may be traced in the characteristic expressions, "I hear something!", later, "I think I hear the fundamental!" and finally, "I am sure I hear the fundamental!"

TABLE II. Average results of M.C.C.

	10 cm.		5 cm.		2.5 cm.	
v.d.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.
18	.13	.04	.40	.13	1.33	.51
17	.23	.04	1.07	.31	2.93	1.18
16	2.00	2.00	3.43	1.05	5.50	2.20
15	2.23	1.78	3.43	1.91	5.33	1.98
14	3.07	1.76	3.50	3.33	9.80	4.20
13	5.23	1.96				

TABLE III. Average results of C. B.

	10 cm.		5 cm.		2.5 cm.	
v.d.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.
18	.23	.18	.90	.47	2.20	.93
17	.43	.18	1.27	.49	3.27	.18
16	.87	.29	1.97	.38	4.00	.67
15	1.40	.13	2.63	.04	2.57	.42
14	2.33	.58	3.07	.29	5.60	.53
13	2.87	.91	4.53	.49		
12	4.50	.67				

TABLE IV. Average results of F. O. S.

	10 cm.		5 cm.		2.5 cm.	
v.d.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.
18	.47	.24	2.47	.78	3.13	.62
17	1.23	.73	3.07	.18	5.07	1.29
16	1.60	.73	4.03	1.24	6.50	.87
15	3.43	3.35	4.60	1.20	7.37	.78
14	3.73	.79	6.00	1.67	9.00	2.33
13	3.70	.53	4.07	.89		
12	3.77	1.34				

TABLE V. Average results of L. E. W.

	10 cm.		5 cm.		2.5 cm.	
v.d.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.
18	1.13	.57	1.77	.38	2.43	.78
17	1.50	.20	2.30	.60	3.47	1.51
16	2.73	.84	3.23	.62	5.33	.58
15	2.70	.20	4.63	.91	7.00	1.00
14	4.17	1.42	4.13	.24	6.40	1.40
13	5.40	2.13	7.07	.76		
12	6.10	.60				

TABLE VI. Average results of T. F. V.

	10 cm.		5 cm.		2.5 cm.	
v.d.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.
18	.67	.04	1.13	.24	1.90	.27
17	1.23	.18	1.90	.13	3.13	.58
16	1.27	.18	2.40	.27	3.87	.44
15	1.77	.18	3.60	.27	5.50	.00
14	3.17	1.22	5.53	1.29		
13	5.67	.44				

TABLE VII. Average results of five observers

	10 cm.		5 cm.		2.5 cm.	
v.d.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.	Ampl. in mm.	m.v.
18	.54	.08	1.33	.24	2.20	1.05
17	.93	.09	1.92	.21	3.57	.72
16	1.69	.73	3.01	.58	5.04	.60
15	2.31	.33	3.78	.61	5.55	.52
14	3.29	1.03	4.45	1.16	7.70	.90
13	4.57	.96	6.37	2.68		
12	4.78	.37				

The quantitative results of this series of experiments are expressed in Tables II-VII and Fig. 1. The records show that, within the limits here studied, the larger area offers more favorable conditions for the perception of the lowest audible tone. To be more specific, the threshold varies inversely as the area of the vibrating surface; *i.e.,* the greater the area, the less the frequency of vibration, or the lower the threshold may be. In all of the curves, whether of individual series, or of averages of series, this law is demonstrated. In every case the frequency of vibration is less with the area of 10 cm. than with the area of 2.5 cm. The frequency of vibration with the area of 5 cm. usually falls between that of the area of 10 cm. and the area of 2.5 cm., but occasionally it is equal to one of these, but even then there is a decided difference in amplitude. The results show that the average threshold for the first five observers lies between 15 and 14 v.d. with the smallest area,

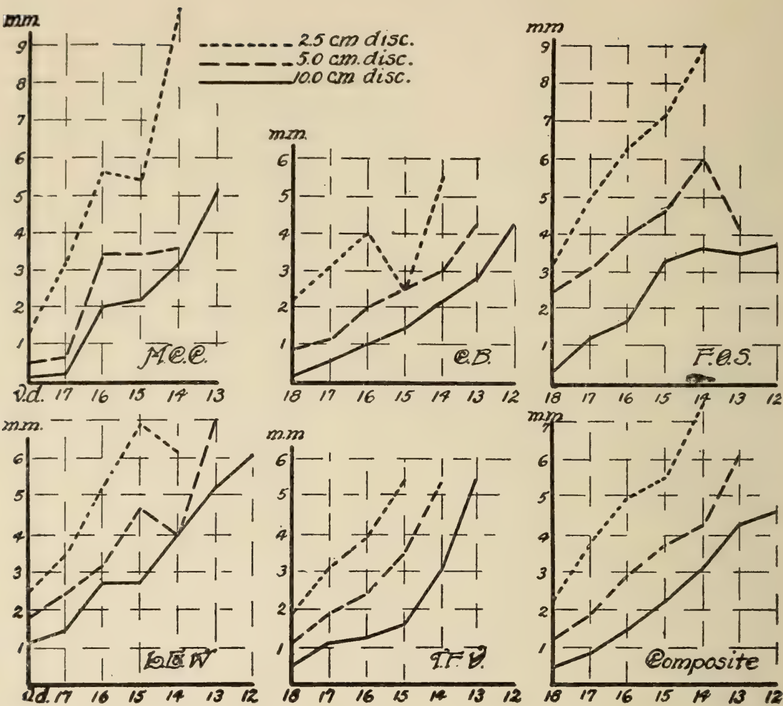


Fig. 1. (Tables. II-VII.)

between 14 and 13 v.d. with the middle area, and between 13 and 12 v.d. with the largest. This is as accurate a determination as can be made from the data secured. There are, obviously, certain judgments which cannot be accepted at their face value. The average results of F. O. S., as shown in Figure I, may be taken as an illustration. In this case there should be much hesitancy in placing the threshold at 13 v.d. for the middle area inasmuch as the amplitude required is considerably less than that required for 14 v.d. The drop in the curve for the smallest area for L. E. W. may be interpreted in a similar way. In either of the instances cited there is, however, the possibility that the observers actually did perceive a tone, the small amplitude being the result of better attention.

Within limits, then, the threshold is lowered by increase in area. This limit, under the conditions of the experiment, is approximately 10 cm. Results secured by Mr. Imai with a disc of 12 cm. in diameter are no better than those secured with a disc of 10 cm. Furthermore, at the beginning of my own investigation, the area was

increased to 20 cm. but the conditions were in no way improved by this still larger area.

Again, the threshold, as measured in terms of frequency of vibration, varies inversely as the amplitude of vibration; in other words, increase in amplitude lowers the threshold, within limits. It requires a greater amplitude to produce an audible tone of 17 v.d. than it does to produce one of 18 v.d. And so the amplitude gradually increases until the threshold is reached. Thus the curves have an almost uniformly ascending trend. Variations in attention offer the most probable explanation for the exceptions that occur in the lower part of the curves, but the confusion of the overtone with the fundamental is the most frequent cause of the irregularities in the upper part.

Mr. Imai conducted two sets of experiments on the variable of distance. From the first of these he deduced the law that, within limits, the maximal distance at which the fundamental tone is just perceptible varies directly as the pitch; *i.e.,* the higher the pitch the greater the distance it can be heard.

In the second series he used two different discs to vary the area, one 12 cm. in diameter and the other 6 cm. The difference in distance which resulted from this difference in area he found very slight,—not more than 1 mm. But this difference, together with incidental observations recorded throughout the course of the whole experimentation, led him to the conclusion that, within given limits, the maximal distance at which a threshold tone could be perceived varies directly as the area of the vibrating surface. Both of the above conclusions have been verified by our own experimental procedure.

The variables of a subjective nature encountered in the attempt to determine accurately the lower limit of tonality are not different in kind from those which are met in experimenting upon any other part of the tonal range. But the fleeting, momentary character of the low tones makes it extremely difficult to hold them in clear consciousness. Certain of the subjective variables, therefore, become more disturbing in the lower register than they do in the middle register. Every precaution must be taken to control them. Individual differences and variations within the individual have a vital bearing upon the problem.

Practice is one of the most significant variables but fortunately

it can be easily controlled. The difficulty of the task set the observer necessitated in every case a rather high degree of practice. When the series of experiments opened the observers had first to learn to distinguish between fundamental and overtone, and later, in the case of very low tones, to distinguish between fundamental and puffs of air with comparative ease and certainty. The records of three of the observers show clearly the effect of practice also in the lowering of the record. Musical education undoubtedly lessened the amount of special training required. Mr. Imai observed this also in his experiment. A few members of the Minneapolis Symphony Orchestra were given a series of these tests and nearly all of them heard the tone of 12 v.d. This superiority of musicians may be due to a selection—to a musical nature rather than to training, although both count.

Fatigue can easily become one of the most vitiating factors. Its effect is both physical and mental. It has been demonstrated in this laboratory that tones in the middle register may be listened to attentively for two hours without a disturbing effect. But low tones fatigue the ear very quickly and when thus incapacitated the power of analysis deteriorates and the determination of the threshold therefore becomes uncertain. Furthermore, it demands the closest attention on the part of the observer to sense the fundamental tone. There is, of course, a strain of attention and when mental fatigue occurs sensations and images of tones may be confused. It is highly essential, therefore, that the experimental series be of short duration and that it should be interrupted by frequent periods of rest.

The observers had a strong tendency to give the tone a definite location in space. One or two placed it in the back part of the head, very near the neck. One said that when he heard both the fundamental and the first overtone, the fundamental had a lower place in space than did the overtone. Another maintained that the first tone heard was usually an overtone and seemed very close to the ears, but the fundamental seemed to come out of the darkness from some place lower than the ears.

The larger area produced by the attachment of the discs caused puffs or whiffs of air to occur in connection with the tone. The larger the area, the more noticeable they are. Very early in the experimentation the puffs confused the observers, but after practice

they were scarcely noticed until the threshold had been reached when nothing but the puffs remained. They bear the same relation to the lowest audible tone that the thud, produced by striking the mallet against the König cylinder, bears to the highest audible tone. The observer ignores the thud until the threshold has been reached.

With three observers the fundamental bore a temporal relation to the overtone. The overtone was nearly always heard first, then the fundamental came out gradually from under the overtone, remained for an instant and then disappeared leaving nothing but the puffs. One observer said, "The fundamental arises as a faint impression in the ear and dies out quickly. It seems to emerge from beneath the overtone while the latter ceases. It does not come to consciousness suddenly, but gradually."

Low tones are intrinsically weak; therefore any factor that would increase the energy of the wave motion would tend to make a low tone more audible. Distance, *i.e.,* nearness to the ear, does this; area does also. In so far as mere audibility of the tone is concerned, amplitude, nearness, and area are the chief factors. But the element under observation is fusion of the individual vibrations into a tone. What has been brought out most clearly in this investigation is that the limit of fusion is set not only by vibration frequency but by the factors just named. Their total effect should be interpreted primarily in terms of the *form* of the wave. Nearness, large amplitude, and large area all unite to form a continuity of the wave. When the waves which impinge upon the tympanum assume the form of the sine curve we have probably the most favorable conditions for that fusion which is the essence of tonality.

In summary, the factors which must be taken into account in the accurate determination of the lower limit of tonality are of two kinds, the objective and the subjective. The objective factors are four: namely, area, amplitude, distance, and timbre. It has been shown (1) that the threshold varies inversely as the area of the vibrating surface; (2) that the threshold varies inversely as the amplitude of vibration; (3) that the amplitude of vibration varies inversely as the area of the vibrating surface; (4) that the maximal distance at which the fundamental tone is just perceptible varies directly as the pitch; and, (5) the maximal distance at which a threshold tone can be perceived varies directly as the vibrating surface. The subjective variables are in general the same as those

encountered in other experiments in audition. They are, differences in innate capacity, in degree of practice, in ability to concentrate attention and in mental content. The wide variation in threshold-values, as determined by the different investigators, may be accounted for by the lack of control of some of these variables.

VARIATION IN PITCH DISCRIMINATION WITHIN THE TONAL RANGE

BY

THOMAS FRANKLIN VANCE

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HISTORICAL STATEMENT

The object in this brief historical survey is to place before the reader only those results which are most closely related to the present problem with respect to the methods used and the aspects investigated. Therefore, investigations made with similar methods but having only a narrow field, as well as those exploring a large range by different means, will not be discussed.

Preyer may be considered the pioneer worker on the problem of pitch discrimination within the tonal range. Earlies investigators concerned themselves with the least perceptible difference at only a single point in the register. Delazenne, (1) in 1827, used a metal string 1147 mm. in length with a vibration rate of 60 v.d., which he divided into equal parts by a bridge. He found that if the bridge was removed only 1 mm. from the central position a difference of pitch could be detected by well trained ears when the two parts of the string were sounded in succession. Weber (20) declared that he was able to determine the pitch of tones so accurately through the ear that he never made an error of more than one vibration on a tone of 200 v.d. He thought it possible that this keen discrimination

might be due to beats. Sauveur (9) with two monochords tuned to the same pitch found that when one string was shortened by 1/2000 of its length a difference in the pitch of the two tones was recognizable, but he leaves no record as to the pitch of the tones. Schleiber (12) recorded a differential threshold of less than .5 v.d. on the tone of b. Seebeck (14) and two superior violin players differentiated without fail between two tuning forks vibrating at 1209 and 1210 v.d.

Preyer.—Preyer (7) used the Appunn tonmesser,—an instrument consisting of reeds which gave the following tones: from 500 to 501 in steps differing by 0.1 v.d., 504, 508, 512, 1000 to 1001 in steps of 0.2 v.d., 1008, 1016, 1024, 2048, and 4096 v.d., respectively. More than a thousand judgments as to whether the two tones compared were equal or different, were secured from twelve practiced observers. They were not required to tell the direction of the difference. To his own results he added those of Delezenne and Seebeck. Transcribing Delezenne's measurement in millimeters into terms of frequency of vibration, he found that the shorter string produced a tone of 120.2 v.d., while the longer one gave a tone of 119.8 v.d., thus making a difference of 0.4 v.d. which skilled observers could detect. Likewise, a simple computation revealed the fact that Seebeck had the very low threshold of 0.36 at 449 v.d. After some verification of Seebeck's results, Preyer inclined to the belief that the threshold might be brought as low as 0.25 v.d. The combined results give differential thresholds for four different places in the tonal range as follows: Delezenne at 120 v.d. found the threshold to be 0.4 v.d., Seebeck at 440 v.d. found it to be 0.4 v.d., Preyer at 500 v.d., 0.3 v.d., and at 1000 v.d., 0.2 v.d. From this study, inadequate as it may seem, Preyer draws the following conclusions:

(1) One-third of a vibration on 500 v.d. and five-tenths of a vibration on 1000 v.d. will always be recognized as different by the best observers, although the most sensitive, the most trained, and the most reliable ears tested could not recognize a difference of 1000 and 1000.25 v.d. nor of 500 and 500.2 v.d. (2) Very high tones and very low tones cannot be discriminated so accurately as tones of the middle region. Capacity for discrimination is keen between c and c^3, keenest for the region from a^1 to c^2, but beyond c^3 it decreases slowly until it becomes very unreliable at c^5. Fis^4 marks a second point of keenness of capacity. (3) The relative difference for pitch is dependent, in a high degree, on the number of vibrations

of the compared tones; and the absolute difference-sensitiveness does not decrease with the pitch. (4) The judgment concerning the place of tones in the tonal line is more uncertain than the judgment as to whether the two tones lie at different points. (5) Practice is an influential factor in pitch-discrimination. Extreme fineness of capacity is peculiar only to those who have much familiarity with tones.

Luft.—In the Leipsic laboratory, during the years 1884 to 1886, Luft (4) experimented upon a range of tones extending from 64 to 2048 v.d. Tuning forks were used for the production of the tones, the variable being mistuned from the standard by means of sliding weights. The forks 64, 128, and 256 v.d. were energized by the stroke of a hammer of India-rubber, forks of 512 v.d., by means of a violin-bow, and forks of 1024 and 2048 v.d., by a wooden hammer, which was padded with felt. Forks of 64 v.d. were held upon large resonator-boxes; 128, 256, and 512 v.d. were brought to the openings of resonator-tubes of paper; while 1024 and 2048 v.d. were permanently attached to resonator-boxes. He used the method of minimal change and employed from four to eight different steps in passing from a large difference to one that was just perceptible, with the following results:

Standard v.d.	64	128	256	512	1024	2048
Difference	.15	.16	.23	.25	.22	.36

"In the field of tonal quality within the region investigated the psychophysical law, according to which the absolute differences of sensation correspond to relative differences of stimulus which must be constant, finds no application. On the contrary the differential threshold within the interval mentioned approaches the constant average of 0.2 vibrations."

The threshold value slowly rises from 64 to 2048 v.d. with the single exception of 1024 v.d. Luft admits that the error here is probably due to an objective circumstance. His results can scarcely be compared with those of Preyer as the methods employed were quite different. It is a point worthy of observation, however, that with Preyer the point of finest discrimination lies at 500 v.d. while with Luft it lies at 64 v.d. Later, Luft found the threshold of 32 v.d. (by the same method employed with 64 v.d.) to be about .44 v.d. His results, therefore, do not contradict Preyer's statement that very high tones and very low tones cannot be distinguished as readily as tones of the middle register.

Luft noted that practice lowers the threshold and that the effect

of practice is not equally distributed in all parts of the range. The influence of practice is of special importance at 64 and 128 v.d. Luft lowered his own record from 0.85 to 0.3 v.d. at 128 v.d., and from 0.42 to 0.15 v.d. at 64 v.d. He believes that the only reason that the initial thresholds for the lower tones is higher than those of the central region, is, that the degree of practice for the former is not so great. The variable of practice was far less noticeable in the higher regions which was due, he believed, to the greater intensity and persistence of these tones. He even ventured to say that the individual variations of the differential threshold are, for the most part, due to practice.

Meyer.—In 1898, Meyer (5) published Professor Stumpf's thresholds for the discrimination of pitch. Tuning forks were used for the tones 100, 200, 400, 600, and 1200 v.d. The variable forks were mistuned by the insertion of a screw in the end of the prong,—a more accurate device than that of sliding weights. After discarding the method of minimal change as practically worthless for the problem in hand, and thereby questioning the validity of Luft's results, Meyer adopted the method of right and wrong cases. Forks of 100 and 200 v.d. were held in the hand and brought to the openings of the resonators, while forks of 400, 600, and 1200 v.d. were mounted on resonator-boxes and were energized by the blow of a hammer. Each individual experiment was performed three times and even more if the observer wished it, before a judgment was required. In this manner Meyer thought to equalize variations of intensity and time-interval, as well as fluctuations of attention. Stumpf's thresholds as determined by means of the Cattell-Fullerton table, from the data given, are as follows:

V. D.	100	200	400	600	1200
Differential threshold	.54	.25	.28	.24	.69

The author concluded his report thus:

"One sees, therefrom, that approximately the same difference of pitch is recognized with equal certainty at 200, 400, and 600 v.d. and with less, but likewise moderately equal certainty, at 100 and 1200 v.d. The differences in these cases are so small that they may be considered accidental. That the certainty of judgments declines in still higher and still lower tones is self-evident."

Stücker.—Stücker's work (16) is the most extensive study published on this particular subject. His observations covered the range between the limits 72 and 35000 v.d., or nine entire octaves. He

employed the following standard tones: d^{-1} (73.4), c^0 (130.5), c^1 (261), all the tones of the major scale up to c^2 (522), a^2 (870), a^3, g^3 (3100), c^5, g^5, c^6, g^6, c^7 and c^8. All of the tones up to and including c^2 were produced with tuning forks, a^2 and a^3 with a monochord, and the remaining ones with a Galton whistle. In each individual series he started with a large difference in the number of vibrations of the two instruments and then made the difference gradually smaller until the threshold was reached. Such a procedure was repeated a few times for the purpose of verification. Whether the observer indicated the direction of the difference or merely the difference is not stated. Given below are the average values of the relative and absolute sensitiveness of discrimination of his fifty observers for eight different levels, with his statement in summary:

Pitch	d^1	c^0	c^1	a^1	a^2	a^3	g^3	g^5
Rel. Disc.	.94	.74	.49	.32	.30	.44	.86	4.91
Abs. "	.7	1.	1.3	1.4	2.5	7.7	26.7	304

(1) Neither the absolute nor the relative sensitiveness to difference of the two tones remains constant in the different tonal regions. (2) The relative difference-sensitiveness is in general the greatest in the first and second accented octaves; in many cases, however, the second maximum lies in the third and fourth accented octaves. (3) With one-third of the entire number of observers the relative sensitiveness to difference in the second half of the first accented octave is nearly equal; namely, 0.2 and 0.3; when one compares the individual curves of sensitiveness with these, the places of greatest sensitiveness lie in the upper half of this region, while with unmusical individuals they occur in general in the lower half. (4) The degree of sensitiveness is subjected to fluctuations within an octave, which is repeated in each octave in the same proportion; it is the greatest for c, slightly less great for g and still less for f and h. (5) A number of persons possess a secondary maximum of sensitivity. (6) An unusually great sensitiveness in high tonal regions is a characteristic of musical persons.

Stücker points out that the discrimination was far more accurate in the lower regions when the second tone was lower, while in the higher region the opposite was true. The inference here is, that judgments are facilitated when the second tone is farther removed from the first and second accented octaves, which are most frequently employed in musical composition; i.e., when the second tone is the farther from this middle register, the judgment seems to be more accurate. He further adds, that the daily variation of non-musical observers is less than for musical ones.

A year later this same author (17) published a report supplementing the one just reviewed. In this he states the results obtained from three different types of observers, professional players of various instruments, singers, and individuals decidedly unmusical. The average values of the absolute differences for the three different sets of observers have been computed for seven levels in the tonal range, as follows:

	d^1	c^0	c^1	a^1	a^2	a^3	g^4
Players	.35	.37	.40	.56	1.20	2.64	13.0
Singers	.46	.48	.44	.71	1.62	3.07	14.0
Unmusical	12.62	2.20	2.80	4.80	9.96	24.00	130.0

Of special interest in this second article is the statement that with tenors and sopranos the finest discrimination is found beneath their voice register, but with bass and alto singers above their voice register; the difference is not between the voices of men and women, but only appears between the relative height and depth of the voice-register of both sexes.

The age difference, he maintains, is more significant than that between musical and non-musical observers. After the age of thirty, sensitiveness to difference declines and the range becomes restricted.

Schaefer.—In 1910, Schaefer (10) submitted a thesis to the Department of Psychology of the State University of Iowa on the subject, "The Curve for the Variation of Pitch Discrimination within the Tonal Range", which has not been published. The apparatus and method were practically the same as those used in the present investigation. For observers, he had fifteen normal individuals varying in musical ability and training. Five hundred tests were given on each of the tones 24, 32, 64, 128, 256, 512, and 2048 v.d. The average threshold for each of these in the order given is as follows: 3.3, 3.4, 2.9, 1.3, 1.5, 1.8, and 6.7 v.d. He summarizes his results thus:

(1) The form of the composite curve indicates that discrimination for the average normal individual is most difficult in the higher and the lower registers and becomes easier in the middle register. (2) The majority of the individual curves are of the same form as the composite. Curves of individuals having high thresholds are of about the same form as the curves of individuals having low thresholds. (3) There are notable individual differences. (4) Musical training does not influence to any large extent, the ability to perceive difference of pitch. (5) It is easier to detect difference in pitch than to name the direction of the difference.

STATEMENT OF THE PROBLEM

The primary purpose of this investigation has been to determine the prevalence of islands or gaps in pitch-discrimination within the tonal range. The pursuit of this aim has taken the form of an attempt to make a comparatively large number of complete individual measurements on pitch-discrimination within the tonal range with as many as possible of the hitherto unknown or disregarded sources of error under control. On the basis of frequently observed defects in the hearing of pitch, found in clinical cases, it is generally believed that such disturbances occur in varying degrees in normal persons. In the curves of two or three of Schaefer's observers, there are places where discrimination of pitch is less keen than the balance of the curves would seem to indicate that it ought to be; in the case of one observer the evidence of a gap was striking. Professor Titchener (19) deems such cases of sufficient importance to bring to the support of the Helmholtz theory of hearing. He says:

"Cases occur in which the range of hearing is normal, but the tonal scale is not continuous; there are tonal gaps, large or small, parts of the scale where the patient is completely deaf to tonal stimuli, though he can perfectly well hear the cases above and below."

The sources of error in a problem of pitch-discrimination are so great and insistent that successive investigators of the same problem are fully justified in a patient struggle to overcome them with progressive insight. In reading the various reports on the subject, one cannot help being impressed with the fact that very few, if any, of the investigators fully realized the significance of the many important variables which could easily—and doubtless did—vitiate the results. The disturbing factors, due to faulty apparatus and inadequate procedure, mentioned by Professor Seashore in his preliminary report (13), suggest the seriousness of the problem. From my own experience I am convinced that his statement in regard to these factors is in no way exaggerated. Rather, it has not been made sufficiently emphatic. The danger of false criteria entering into the judgments of the most conscientious observer, either consciously or unconsciously, can scarcely be realized by one who has not encountered them first-hand. The danger of identification, alone, is sufficient to make the investigator very cautious.

Apparatus and Method.—In this investigation the measurements

were made at six different levels in the register; namely, 64, 128, 256, 1024, and 2048 v.d. The tones were produced by the best grade of Kohl tuning forks. For 128, 256, 512, and 1024 v.d. Helmholtz resonators were used; the forks of 2048 v.d. were mounted on resonator-boxes; while resonance for 64 v.d. was produced by extending the Helmholtz resonator for 128 v.d. For 64 v.d. a second set of forks was found to be more satisfactory at a later stage of the experiment. These were made of round tool steel, 12 mm. in diameter. The prongs were 30 cm. in length and carried hard rubber discs 10 cm. in diameter.

The sounder was a simple device consisting merely of a lead pipe about one inch in diameter with one end bent into the form of a circle for the base, and the other in the shape of a U at right angles to the base. The U-end, when covered with several thicknesses of rubber, made a sounder of the required elasticity and softness. The placing of the sounder on leather sand-bags resting on a heavy metal stand eliminated, in large part, the accessory noise of the blow. The forks of the four central octaves were energized by striking the middle of the prong upon the sounder; the forks of 2048 v.d. were struck as lightly as possible with a felt-hammer; while those of 64 v.d. were set into vibration by striking them on the sand-bags.

To mistune the variable fork, in every case except those of the lower limit, a screw was inserted in the end of each prong and to these were attached nuts, varying in weight, to give the desired pitch. Such a device is a decided improvement over the method of sliding weights, inasmuch as the latter may allow a slight change in position, with a corresponding change in pitch, during the course of the experiment. This is especially true of the smaller forks. At 64 v.d. variation in pitch was secured by shifting the discs, which were firmly attached to the forks by large set screws. At each of the steps the successive differences of one, two, three, five, and eight vibrations were chosen—a range which was found to be sufficient for all but one or two observers. All of the forks were tuned to an accuracy of five-hundredths of a vibration per second.

The mode of procedure followed the plan suggested by Professor Seashore in his preliminary report (13) in almost every respect, in the four central octaves where it was possible to do so. A most careful attempt was made to keep the tones at a constant intensity

without resorting to the uniformity of mechanical devices. The experimenter simply relied on the accuracy of his own hand and ear in presenting the forks in such a way that the tones would be of equal strength. If at any time, through a lapse on the part of the experimenter, the difference of intensity seemed pronounced, the trial was repeated. Mechanical devices are particularly unsatisfactory in that the difference of intensity which is practically certain to occur, be it ever so slight, is constant and might thus become a criterion for identification. In the method of presentation by hand, this source of error is eliminated. The ideal presentation is that in which the tones are just loud enough to be heard without a strain of the attention, and extreme care was taken throughout to guage the tones by this standard. The duration of each tone, as well as the time-interval, was approximately one second. Whether the constant or the variable tone should be presented first, was decided by a key which had been arranged first by chance and then revised to the extent that the same order could be followed no more than three times in succession, and that in one hundred tests the two possible sequences should have the same frequency. The observers in every case were required to render their judgments in terms of "second tone lower", or "second tone higher", in accordance with the method of right and wrong cases. No doubtful judgments were allowed; when the observer felt uncertain after repeated tests he was simply requested to guess. As a rule each individual experiment was given but once, but whenever disturbances of any sort, either objective or subjective, were noted, the experiment was repeated. Observers were instructed to trust the first impression. Except with the lowest tones, where the judgments were given orally and were then recorded by the experimenter, the observers themselves kept the record by simply writing *H* or *L* as an abbreviation of the judgments "higher" or "lower." With one observer, however, the response was oral throughout because attention to the writing caused too much of a distraction. At least one hundred judgments were recorded at each level, but many observers required a considerably larger number before their thresholds could be satisfactorily determined. No series of observations extended long enough to cause any disturbing fatigue. The monotony of the experiment was broken at intervals by the checking of the record and by the adjusting of the forks. Fatigue caused previous to the experiment could

not be very well controlled as the observers had to be taken at times which best suited their convenience. The tests were, however, fairly well distributed throughout the hours of the day and those observers who did come at a late hour were always dismissed if they felt fatigue to a degree which they thought might interfere with their best work. The experiment was conducted in the sound-proof room and in every instance the observers were tested individually so that distractions of an objective character were reduced to conditions connected only with the actual experiment.

The experimental control was naturally most difficult at 64 v.d. The large size of the forks not only made them more difficult to handle but also increased the possibility of overtones. Still another problem was presented in obtaining sufficient resonance for these tones of low intensity. Overtones were especially distracting with the first pair of forks that were used, but it was possible to overcome them to some extent by setting the forks in heavy handles of iron and by putting heavy rubber bands upon the prongs. Yet the increased weight added to the difficulty of handling. Two different methods were tried with these forks; namely, bringing the forks to the openings of the resonators, described above, and presenting them to the ear of the observer without the aid of a resonator. Both of these methods are unsatisfactory. The resonator scarcely makes the tones loud enough to make the judgment one of certainty, and it is difficult for the experimenter to present the tones so that they are of equal intensity. Holding the forks to the ear has the advantage of making the tones louder, but here again the variable of intensity is left uncontrolled, and the possibility is open to the observer for obtaining clues from the position of the fork, from timbre, and from noises caused by movements in presentation. The fact that the tones could be distinctly heard by the second method gave it the preference. But when the results were compared with those obtained for 128 v.d. the thresholds seemed abnormally large. This pair of forks was therefore discarded for the forks with the discs, which were found to answer the purpose much better, for at least three reasons; namely, they were freer from overtones, the tones were louder and clearer because of the increased vibratory surface offered by the discs, and they were neither so heavy nor so long, which facilitated handling very materially. With these forks the method of presentation to the ear was adopted, but on

account of the louder tone it was possible to hold the forks farther from the ear. Being also lighter in weight, they could be energized in a more uniform manner, and it was easier to bring them more nearly to the same point opposite the ear; thus the variable of intensity and direction of source could be more adequately controlled. An opportunity was not offered for the retesting of all individuals whose thresholds had been determined by the first pair of forks, but in most cases where a second was possible, somewhat lower thresholds were obtained.

No particular comment in regard to the forks of 128 and 256 v.d. is necessary. They were energized and presented to the resonators with the conditions of duration, time-interval, and intensity carefully controlled. In each case the tones were perfectly clear and distinct. The forks producing these tones held up long enough to allow five individual experiments without restriking. But the control was not quite so satisfactory at 1024 v.d. The forks at this level would vibrate with sufficient energy for only two tests. A more forceful blow was also required, and it was necessary to bring them very close to the small resonators, indeed so close that they nearly touched it. All this, of course, made it more difficult for the experimenter to maintain a constant intensity. Again, the piercing character of the tone was annoying to some observers. The tones produced by forks of 128, 256, and 512 v.d. were not heard by the observers except when reinforced by the resonators. But the 1024 v.d. forks gave a high piercing tone before being presented to the resonator. The observer, as much as possible, ignored this tone and concentrated his attention on the tones as they were intensified by the resonators.

In the upper limit, the method was necessarily quite different. The small resonator-boxes on which the forks were mounted were held in the hand; the one fork was struck and dampened, and then the second in close succession. So delicate a stroke was necessary to produce a tone that the noise of the blow was but a slight distraction, if any. It was extremely difficult, however, to keep the intensity constant. To eliminate discrimination of the direction of source, the position of the left hand was shifted to bring the forks to exactly the same place before they were energized.

Of the fifty observers who made this study possible by giving it their time and thought, thirty-three were members of the elemen-

TABLE I. Absolute differential thresholds

Obs.	64 v.d.		128 v.d.		256 v.d.		512 v.d.		1024 v.d.		2048 v.d.	
	T	m.v.	T	m.v.	T	m.v.	T	m.v.	T	m.v.	T	m.v.
1	2.5	0.9	1.9	0.5	0.8	0.6	0.7	1.1	3.3	.0	5.3	0.4
2	3.5	0.1	0.8	0.6	0.6	0.8	1.0	0.8	2.2	1.1	2.5	3.2
3	3.3	0.1	0.6	0.8	0.7	0.7	0.8	1.0	2.5	.8	5.6	0.1
4	3.0	0.4	1.5	0.1	2.2	0.8	2.3	0.5	1.1	2.2	3.5	2.2
5	4.0	0.6	1.3	0.1	1.1	0.3	0.9	0.9	4.1	.8	7.3	1.6
6			0.7	0.7	0.8	0.6	1.2	0.6	2.4	.9		
7	4.0	0.6	0.7	0.7	1.2	0.2	1.7	0.1	3.5	.2	6.7	1.0
8	1.0	2.4	1.5	0.1	1.4	0.0	1.8	0.0	3.7	.4	6.5	0.8
9	6.4	3.0	2.7	1.3	0.7	0.7	1.7	0.1	5.3	2.0	6.5	0.8
10	1.5	1.9	1.4	0.0	1.1	0.3	1.8	0.0	2.4	.9	4.9	0.8
11	2.0	1.4	0.8	0.6	1.5	0.1	1.0	0.8	3.9	.6	5.6	0.1
12	3.7	0.3	0.7	0.7	1.5	0.1	2.4	0.6	4.3	1.0	3.5	2.2
13	1.5	1.9	1.0	0.4	0.7	0.7	2.0	0.2	3.8	.5	6.7	1.0
14	4.0	0.6	2.7	1.3	1.8	0.4	2.2	0.4	5.0	1.7	10.0	4.3
15	3.0	0.4	1.4	0.0	2.1	0.7	4.4	2.6	6.4	3.1	8.8	3.1
16	3.4	0.0	1.3	0.1	0.6	0.8	0.8	1.0	2.2	1.1	3.0	2.7
17	2.0	1.4	1.1	0.3	2.9	1.5	5.0	3.2	5.1	1.8	7.6	1.9
18	0.7	2.7	1.0	0.4	2.0	0.6	0.6	1.2	0.8	2.5	5.8	0.1
19	5.2	1.8	0.6	0.8	1.1	0.3	0.8	1.0	4.1	0.8	9.7	4.0
20	1.0	2.4	1.0	0.4	0.7	0.7	1.2	0.6	3.2	.1	6.1	0.4
21	3.8	0.4	2.3	0.9	1.6	0.2	1.7	0.1	3.6	.3	5.5	0.2
22	2.5	0.9	0.8	0.6	1.0	0.4	1.3	0.5	2.0	1.3	3.0	2.7
23			2.0	0.6	1.3	0.1	1.4	0.4	6.8	3.5	5.5	0.2
24	6.4	3.0	1.5	0.1	1.1	0.3	1.5	0.3	2.0	1.3		
25	4.0	0.6	2.0	0.6	2.5	1.1	2.5	0.7	2.4	.9	4.4	1.3
26	3.0	0.4	0.7	0.7	1.0	0.4	1.9	0.1	1.8	1.5	5.8	0.1
27	4.7	1.3	0.7	0.7	1.5	0.1	1.7	0.1	2.1	1.2	3.6	2.1
28	3.4	0.0	0.6	0.8	0.8	0.6	1.1	0.7	2.3	1.0	3.2	2.5
29	2.4	1.0	2.1	0.7	1.5	0.1	1.6	0.2	8.4	5.1	10.2	4.5
30	2.5	0.9	2.4	1.0	1.3	0.1	1.4	0.4	4.1	.8	5.7	0.0
31	6.4	3.0	2.4	1.0	2.0	0.6	1.5	0.3	3.9	.6	3.0	2.7
32	2.4	1.0	1.0	0.4	1.0	0.4	1.1	0.7	2.2	1.1	4.6	1.1
33			1.7	0.3	1.4	0.0	2.7	0.9	3.2	.1	8.8	3.1
34	3.3	0.1	1.5	0.1	1.5	0.1	1.4	0.4	2.5	0.8	4.9	0.8
35	6.4	3.0	1.1	0.3	1.7	0.3	1.0	0.8	6.4	3.1	9.9	4.2
36	8.8	5.4	1.1	0.3	0.8	0.6	2.2	0.4	3.5	.2	4.8	0.9
37	3.0	0.4	1.5	0.1	1.4	0.0	2.6	0.8	4.4	1.1	10.2	4.5
38	0.7	2.7	0.6	0.8	0.7	0.7	2.1	0.3	1.6	1.7	3.7	2.0
39	7.2	3.8	4.1	2.7	3.2	1.8	1.6	0.2	7.6	4.3		
40			0.9	0.5	1.2	0.2	0.9	0.9	0.7	2.6	4.3	1.4
41	2.4	1.0	0.8	0.6	0.7	0.7	2.1	0.3	1.8	1.5	3.5	2.2
42	4.0	0.6	2.0	0.6	3.2	1.8	4.9	3.1		7.0	7.0	1.3
43	1.3	2.1	0.8	0.6	0.7	0.7	1.1	0.7	1.8	1.5	5.3	0.4
44	3.0	0.4	1.1	0.3	0.4	1.0	1.2	0.6	2.6	.7	3.0	2.7
45	3.1	0.3	1.4	0.0	1.5	0.1	1.4	0.4	3.2	.1	8.0	2.3
46			1.4	0.0	1.3	0.1	2.6	0.8	2.7	.6		
47	3.0	0.4	1.2	0.2	1.6	0.2	1.7	0.1	2.4	0.9		
48	3.0	0.4	1.4	0.0	1.1	0.3	0.8	1.0	2.7	.6		
49	5.0	1.6	4.0	2.6	2.5	1.1	6.1	4.3				
50	1.3	2.1	1.0	0.4	0.7	0.7	1.0	0.8	0.9	2.4	1.2	4.5
Mean	3.4	1.5	1.4	.57	1.4	.5	1.8	.76	3.3	1.31	5.7	1.56
Median	3.00		1.2		1.3		1.5		3.0		5.5	

TABLE II. Relative differential thresholds

Obs.	64 v.d.	128 v.d.	256 v.d.	512 v.d.	1024 v.d.	2048 v.d.
1	.31	.12	.03	.01	.03	.02
2	.44	.05	.02	.02	.02	.01
3	.41	.04	.02	.02	.02	.02
4	.38	.09	.07	.04	.01	.01
5	.50	.08	.03	.01	.03	.03
6		.04	.03	.02	.02	
7	.50	.04	.04	.03	.03	.03
8	.13	.09	.04	.03	.03	.03
9	.80	.17	.02	.03	.04	.03
10	.19	.09	.03	.03	.02	.02
11	.25	.05	.05	.02	.03	.02
12	.46	.04	.05	.04	.03	.01
13	.19	.06	.02	.03	.03	.03
14	.50	.17	.06	.03	.04	.04
15	.38	.09	.07	.07	.05	.03
16	.43	.08	.02	.01	.02	.01
17	.25	.07	.09	.08	.04	.03
18	.09	.06	.06	.01	.01	.02
19	.65	.04	.03	.01	.03	.04
20	.13	.02	.02	.02	.03	.02
21	.48	.14	.05	.03	.03	.02
22	.31	.05	.03	.02	.02	.01
23		.13	.04	.02	.05	.02
24	.80	.09	.03	.02	.02	.02
25	.50	.13	.08	.04	.02	.02
26	.38	.04	.03	.03	.01	.02
27	.59	.04	.05	.03	.02	.01
28	.43	.04	.03	.02	.02	.01
29	.30	.13	.05	.03	.06	.04
30	.31	.15	.04	.02	.03	.02
31	.80	.15	.06	.02	.03	.01
32	.30	.06	.03	.02	.01	.02
33		.11	.04	.04	.03	.03
34	.41	.09	.05	.02	.02	.02
35	.80	.07	.05	.02	.05	.04
36	.11	.07	.03	.03	.03	.02
37	.38	.09	.04	.04	.03	.04
38	.09	.04	.02	.03	.02	.01
39	.90	.26	.10	.03	.06	
40		.06	.05	.01	.01	.02
41	.30	.05	.02	.03	.01	.01
42	.50	.13	.10	.08		.03
43	.16	.05	.02	.02	.01	.02
44	.38	.07	.01	.02	.02	.01
45	.39	.09	.05	.02	.03	.03
46		.09	.04	.02	.02	
47	.38	.08	.05	.03	.02	
48	.36	.09	.03	.01	.02	
49	.63	.25	.08	.10		
50	.16	.06	.02	.02	.01	.01
Average	.4	.09	.04	.03	.03	.02

tary class in psychology in the University, sixteen others were advanced students in psychology, and one other a member of the staff in psychology. It is important to note that the fifty represent a selected group. The thirty-three from the elementary class were chosen from a class of one hundred or more because their differential thresholds at 435 v.d. were less than 8 v.d., as determined from a test given to the class for purposes of demonstration. The advanced students had likewise shown in previous tests that their thresholds for discrimination of pitch were easily less than 8 v.d. Their closer association with the work in the department of psychology also tended to make them slightly better as a group than the elementary students. This basis of selection must be borne in mind in the consideration of the results, for our composite curve is not an average curve; it is superior to the average. It was gratifying to find that all of the observers took keen interest in the problem and made a sincere effort to give the work their best attention. Their knowledge of the fact that they were chosen because of their former good record helped them to maintain an interest.

RESULTS

The Composite Curves.—Table I includes the individual thresholds in terms of the absolute difference of vibrations for the six points in the range. The odd numbers of the observers refer to women, and the even, to the men. The thresholds are given in column T, and the mean variation in column m.v. At the foot of the table are the mean, the median, and the mean variation of the group. In Table II the same records are reduced to the relative threshold expressed in terms of the fractional part of a whole tone, at the respective levels. The figures in italics at the head show the number of vibrations in a whole tone at each of these respective levels. The record of Table I is shown graphically in Fig. 1 and that of Table II in Fig. 2. By an error the decimal point was left out before each of the numbers 1, 2, 3, and 4, in Fig. 2.

There is evidently no essential difference between the mean and the median curves; they run practically parallel throughout their course, coming a little closer together at 256 v.d. than at any other point. But inasmuch as the mean allows the extremes an influence out of proportion to their importance, the median must be considered the truer representative figure.

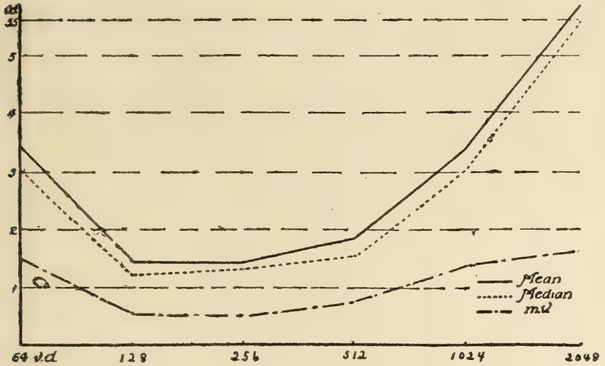

Fig. 1. Mean, median; and mean variation—absolute (Table I).

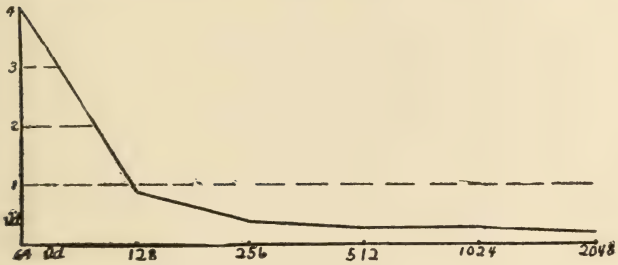

Fig. 2. Mean—relative (Table II).

The average capacity of discrimination, as measured in terms of absolute difference, is practically the same for 128 and 256 v.d. From this central register the curves rise slowly to 512 v.d., from which begins a rapid rise that becomes even more rapid from 1024 on up to 2048 v.d. The curve of mean variation (Fig. 1) follows the general trend of the composite, which means that the thresholds for this group of fifty form a more compact grouping in the central register, while at both the upper and the lower limits they are more widely separated.

The relative curve declines rapidly at first and then very gradually, reaching its lowest point at 2048 v.d. In other words discrimination of pitch, as measured in fractional parts of a whole tone, decreases somewhat abruptly from 64 to 128 v.d., but very slowly from that point to the upper limit of the range studied. In the relative curve the minimal value is at 2048 v.d. which is the region of the maximal value in the curve of absolute difference.

The absolute difference of vibration frequency has been adopted as the vehicle of expression in this report, for the reason that it is slightly more concrete and brings out the individual differences more strikingly. Its true relation to the relative must, however, be kept in mind.

The curves represent with a high degree of accuracy, it is believed, the average capacity of a group of observers such as have had a part in this study. But there is little doubt that the form of the curves has been influenced, to some extent, by certain factors other than those of actual discrimination of pitch. There are objective factors which could not be perfectly controlled and which in some cases have led to confusion, but in other cases have resulted in identification. The former necessarily raised the threshold, while the latter lowered it. The subjective variables of attention and practice are important inasmuch as attention is seldom at is best and then only for short duration, and the degree of practice might always be greater. The thresholds are therefore not quite as low as they would be under the most ideal conditions.

With tuning forks, it is impossible to produce as satisfactory a tone at the extremes as in the central register. Discrimination of pitch at 64 and at 2048 v.d. is thus made most difficult and the observer has a tendency to pick up other criteria than pitch upon which to base his judgments. Differences of intensity, change in the direction of the source of sound, and noises accompanying the control of the experiment are the chief factors which cause disturbance. They lead to confusion, rather than to identification, because the method used necessitated their approximately equal distribution between the higher and the lower tones; that is to say, that they occurred in a chance order, were therefore unpredictable, and consequently could not be used as safe criteria for accurate judgments; for example, if an observer was inclined to judge the more intense tone the higher, there would be an increased probability of error whenever the lower tone happened to be more intense. Had the forks been energized by a mechanical device, rather than by the free hand, these variables would have been constant and would have become a means of identification, rather than a source of confusion. At the higher limit it was difficult to keep the tones of equal loudness. The tones produced by the small forks are very fine and persistent, and a slight variation in the forces of the blow produced

a perceptible change in the intensity of the tones, which was often confusing. Whether or not the greater intensity favored a judgment of higher or lower varied with the individual. For some, the pitch being nearly equal, the louder tone was considered the higher, while for others the reverse experience was true.

It is in the lower limit, however, that the most abrupt rise in the threshold is to be found. As has been previously mentioned, various methods of presentation were given a trial, but none of them, excepting with a very few observers, gave results which were comparable with those obtained at 128 v.d. Only two observers had a lower threshold for 64 and for 128 v.d., (Nos. 8 and 18). For observer No. 20 the thresholds for the two tones were the same, while No's. 10, 13, 29, 30, 38, and 50 were the only remaining ones whose thresholds for 64 v.d. did not exceed that of 128 v.d. by more than 0.5 v.d. In other words, forty observers have a threshold for 64 which is more than one-half of a vibration higher than for 128 v.d. That this difference would have been less had it been possible to rule out all the factors of confusion is probable.

But not all of the variables cause confusion. Those which are constant soon come to be associated with one of the two possible judgments and this, in time, brings about a lowering of the threshold. Just what is seized upon as a means of identification one cannot always say. The auditory capacity of analysis is very keen and often the slightest variable which occurs in a particular setting is selected as a clue for the proper response. Slight variations in timbre are among the most frequent sources of identification. It is impossible to make two forks exactly alike and the unavoidable structural difference may be perceived in the nature and composition of the overtones. The forks of the lower limit are particularly susceptible to variation in timbre. If these differences are perceptible, the error of identification is sure to appear. Even with presentation by hand there is the possibility of the experimenter's falling into some characteristic habit of presenting the forks, which may be identified eventually. He may form the habit unconsciously of striking one fork at a different angle from that of the other, or the time-order may have some constant peculiarity which gives a clue.

The errors due to identification are without a doubt the most serious with which the experimenter has to contend. But in an experiment such as this the error of identification is usually discover-

able by comparing the thresholds of one level with those of the other levels. Whenever an observer has a threshold at any particular level considerably lower than the tentative norm would warrant, the chances are, that the error of identification has had a part to play. The record of No. 20 is wanting in the table for 2048 v.d. because he had discovered some criterion other than pitch upon which to base his judgments. In fact he made nearly a perfect record with a difference of one vibration,—a lower threshold than his records at the other levels would warrant. Two other observers had a similar experience in the upper limit, but when the method was slightly changed, they lost their clue and were forced to rely on pitch. In the lowest level Nos. 28 and 40 were influenced in some way by criteria other than pitch, the latter to such an extent that his results were worthless. As has been said, just what criteria were selected by these observers is not known. Their introspections fail to reveal them, the observers contending throughout—and with undoubted conviction—that they were judging on pitch alone. Such illustrations show that the experimenter connot be too careful in his attempt to keep the judgments confined to pitch.

The subjective variables of attention and practice also play more important rôles at the extremes than in the central register. To secure a low threshold at these levels closer attention is necessary and, as these tones are rarely heard, the degree of practice is much less than for tones of the central register. Practice for these tones is only to be had in the laboratory as they are seldom used in musical compositions. Observers Nos. 2, 28, 47, and 50 were the only ones who had the advantage of practice for these extreme tones and their thresholds at these levels are all below the average.

Summing up, we have found that the curve of pitch-discrimination shows the threshold of absolute difference to be keenest from 128 to 256 v.d.; from 256 to 512 v.d. it takes a gradual rise; and from 512 to 2048 v.d., a rapid rise. On the lower side, from 128 to 64 v.d., the rise is very sudden. As expressed by the curve of relative difference, there is a continual decline from the lower to the higher limit; this decline, however, is very rapid from 64 to 128 v.d., much less pronounced from 128 to 256 v.d., and from 256 to 2048 v.d. the curve becomes very nearly a straight line. It will be of interest now to compare the above results with those of other investigators.

Comparative Curves.—Figure 3 represents the composite results

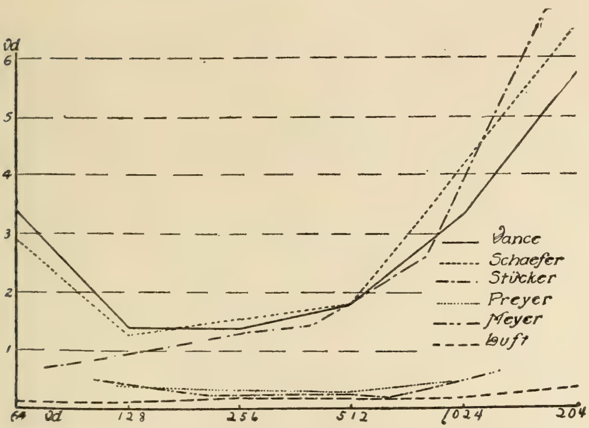

Fig. 3. Composite curves for six different investigators—absolute.

of the six different investigators who have approached the problem by the same general method. The curves of Stücker (16), Schaefer (10), and the writer show considerably higher thresholds than those of Luft, Preyer and Meyer. This difference may be explained. In a pioneer work, such as Preyer's (7), numerous sources of error as yet undiscovered must have had much influence upon the results. His low thresholds can be attributed, in some degree at least, to the error of identification. Since this error may creep in when the best grade of tuning forks is used, there is little doubt but that it must have played an important rôle with the tonmesser. Furthermore this instrument, the reed, is not reliable for fine pitch differences. Luft's (4) values, especially at the extremes, must likewise be questioned. Whenever such low results are obtained at 64 and at 2048 v.d., there must be conclusive evidence that they are not due primarily to discrimination of pitch, but to some other factor which permits of identification. Luft has given no such proof. Furthermore, in a problem of this kind, the method of minimal change, which he used is unreliable, as Meyer has well pointed out, in that it introduces factors other than those of pitch and the threshold value is not quite comparable to the threshold value in our method of constant stimuli. Professor Stumpf's curve, drawn by Meyer (5), shows exceptional ability and is probably accurate. The low thresholds can be adequately explained by the less extended range, by extraordinary natural capacity, and by a high degree of training in

experimental work. On the other hand it should be kept in mind that the curves of Stücker, Schaefer, and the writer, represent the results of a much larger number of observers, many of whom do not have exceptionally fine capacity for discrimination of pitch. The curves are therefore on a much higher level than if they were drawn exclusively from the results of observers who had unusually fine ability.

In the curves of Preyer, Schaefer, Meyer, and the writer the minimal threshold lies somewhere near the central region; but in the other two discrimination seems to be the best in the lowest level. With Preyer the finest capacity is at 500, with Luft at 64, with Meyer at 600 (although the thresholds for 200, 400, and 600 are practically equal), with Stücker at 73.4, with Schaefer at 128, and with the writer at 256 v.d. The maximal threshold is to be found in the highest part of the range in every case. The second maximum lies with Preyer, Meyer, Schaefer and the writer at 64 v.d.

An examination of these curves raises the question as to the cause of the variations. Individual differences are, of course, the principal cause but the nature of the objective control is undoubtedly a very important factor, especially at the extremes. If the apparatus and the method of the three investigators, who had a large number of observers, had been equally refined at the different steps, these grosser differences would probably not have occurred. As it is, they are most pronounced at the extremes where the control was the most difficult. The experimental control at 128, 256, and 512 v.d. can be made so perfect that no observer will be able to pass consistent judgments on any criterion other than pitch. For this reason the results of Stücker, Schaefer, and the writer agree, approximately, within this region.

Inasmuch as the curves take the same general direction, the variations in the upper limit are about what would be expected when one considers the difficulties to be encountered, together with the fact that one of the experimenters used an entirely different apparatus. But from 130.5 to 73.4 v.d., Stücker's curve continues in the same general direction which it has had throughout the entire course, while the other two curves have changed their direction. In other words, Stücker found the absolute difference for 73.4 v.d. to be less than for any other point in the line, while both Schaefer and I found at 64 v.d. the second maximum which is noticeably

greater than for any other point except at 2048 v.d. Luft's results seem to confirm those of Stücker, but Meyer's curve, as well as Preyer's, shows a rise at the lower limit. Indeed the ratio between the thresholds of Meyer for 100 and 200 v.d. is very similar to the ratio between the thresholds for 64 and 128 v.d. obtained by Schaefer and myself. I have no hesitancy in concluding, therefore, that sensitiveness in the great octave is, in general, not so keen as in the small octave. But for reasons already given, it does not follow that the difference is actually as great as the numerical results of this study would seem to indicate. In the light of the experience of the present study, however, Stücker's findings in the lower limit must be held in question. It seems more probable that his observers had learned to make judgments on some criterion other than pitch. Just what that may have been cannot be stated definitely as that author has failed to give any detailed account either in regard to method or to apparatus. It is only known that the tone in question was produced by a tuning fork. The possibilities of error with the large tuning forks are, however, sufficiently great to warrant the statement that Stücker's low record is due, not altogether to discrimination of pitch, but that secondary criteria have been operative in giving the low thresholds.

Individual Differences.—An examination of Table I discloses the fact that the observers may be classified in two general divisions. In the first there are thirty-seven whose curves follow the course of the composite curve in that the smallest values are to be found in the central register on either side of which a slow or a rapid rise is evident. In the second division, are thirteen whose curves do not conform to any general type. In the irregularity of the curves of this second division lies the only possible evidence of gaps which this study has developed.

The curves of the first division may, in a general way, be given a three-fold classification; namely, (1) those which show a relatively low threshold at some point in the central region and relatively high thresholds at the extremes, (2) those in which the thresholds are fairly uniform throughout the entire range, and (3) those curves in which the threshold for 64 is lower than for 2048 v.d.

Division I.—In Figure 4 are the five curves of the first group which show, the most strikingly, the relatively low thresholds in the central register and the higher thresholds at the extremes. These

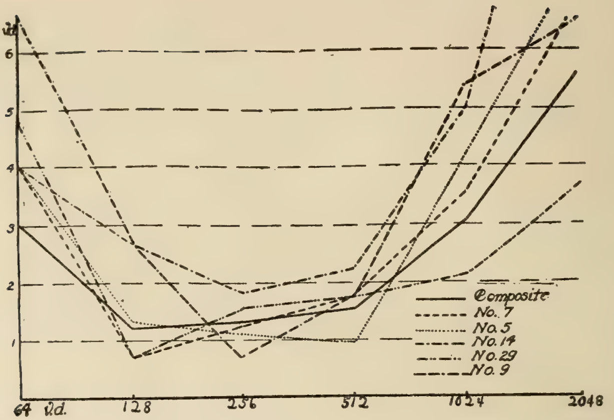

Fig. 4. Individual curves of observers Nos. 5, 7, 9, 14, and 27, which show relatively low values in the central region and high values at the extremes. The solid line is the composite curve of the fifty observers.

curves all resemble the composite more or less closely. At the extremes, however, all excepting that of No. 27 rise above the composite, but in the central region, at 128, 256, and 512 v.d., one-third of the fifteen thresholds pass beneath it. The normal variation of the point of keenest discrimination is well illustrated in this figure. Nos. 7 and 27 made the best record at 128, Nos. 9 and 14 at 256, and No. 5 at 512 v.d. In fact, all but one of the entire number of observers made their lowest record at one of these central levels.

These curves represent the results of observers who were the most unreliable. Very few of these values indicate the physiological threshold. One could not say that the high values at the extremes should be interpreted to mean that all of the observers in question were unable to perceive smaller differences on account of physiological incapacity. It is much more probable that the difficulty is psychological. Individuals of this type do not adapt themselves so readily to new situations under experimental control. When new adjustments must be made their work is relatively poor and continues on a low plane until time has been given for the proper adjustment after which their work may be on a par with that of individuals who adapt themselves more quickly to new situations.

Figure 5 represents the results of the six individuals who are most typical of the second group. All of these observers are men, but they are not of equal rank in previous work in discrimination of

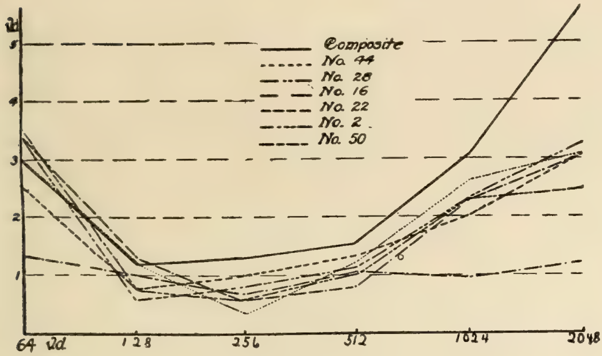

Fig. 5. The composite curve of the fifty observers and the individual curves of Nos. 2, 16, 22, 28, 44, and 50, which are characterized by a high degree of uniformity in the threshold values throughout the range.

pitch. Nos. 2, 32, and 48 were graduate students in psychology and were trained in other tests of discrimination; with the remaining three, however, previous training was very limited. The striking difference between these curves and those of the former group is, as would be expected, with reference to the extremes. The values for 64 and especially for 1024 and 2048 v.d. are lower than in curves of the first type; they approach, therefore, a more uniform level,—a goal which is most nearly approximated by No. 48. In contrast to the former group, these curves fall below the composite at practically every point; only four values are actually higher than the composite, while two more are equal, and these are at the lower limit. From the standpoint of consistency, the curves of Class II can easily be judged the better. Observers who give such results are reliable. With a state of secondary passive attention, they are able to meet the new situation in an easy and natural manner and are little disturbed by unusual difficulties which may be presented. In addition, exceptional ability in analyzing a problem enables them to select the proper element or elements upon which to base their judgments, even though there be disturbing factors. They are so consistent that the experimenter can feel a high degree of assurance that their records represent a close approach to the physiological threshold.

The curves of the five individuals who are most representative of the third group are shown in Figure 6. The peculiar character of these curves, in contrast to those already considered, lies in the lower limit. Here the thresholds are very much lower than for Class I

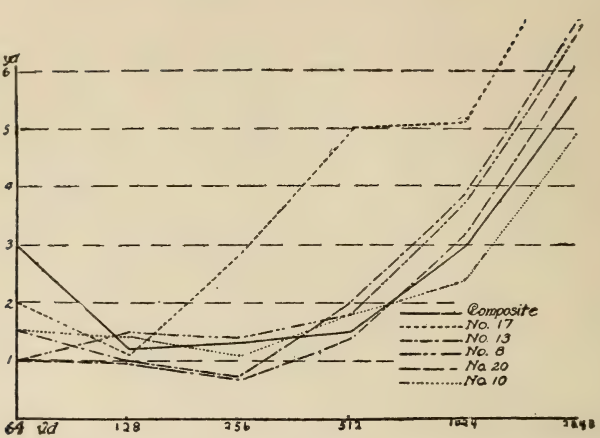

Fig. 6. The composite curve of the fifty observers and the individual curves of 8, 10, 13, 17, and 20, which show relatively low values at the lower limit and high values at the upper limit.

and considerably lower than they are for Class II. But in the upper extreme the curves are similar to those of the first group, with one exception,—the curve of No. 17. Indeed the average results of these five observers form a curve which closely approximates Stücker's curve, the essential difference being, that the latter is tilted at a slightly different angle, due to the fact that Stücker's thresholds at 73.4 v.d. are lower than ours and higher in the vicinity of 2048 v.d.

The similarity of our results to those of Stücker in the lower extreme might invite the same criticism which we advanced against him. It might be said that our low threshold at 64 v.d. was due to the discovery of some variable other than pitch upon which the judgment was based. There is, of course, the possibility that this occurred, but reference to Figure 7, in which the composite curves of the three groups may be compared, leads to the belief that such a criticism does not have much weight with respect to these particular observers. It is to be observed that the minimal thresholds of the first two groups lie at 256 v.d. with a gradual rise on either side of this point. The point of keenest discrimination for Class III, however, lies at 128 v.d. with here again a rise on either side proportional to that which we find in the other two groups. In other words, the form of the latter curve from 64 to 256 v.d. is similar to the form of the other two from 128 to 512 v.d. We should expect to find a higher threshold for 64 when the minimum is

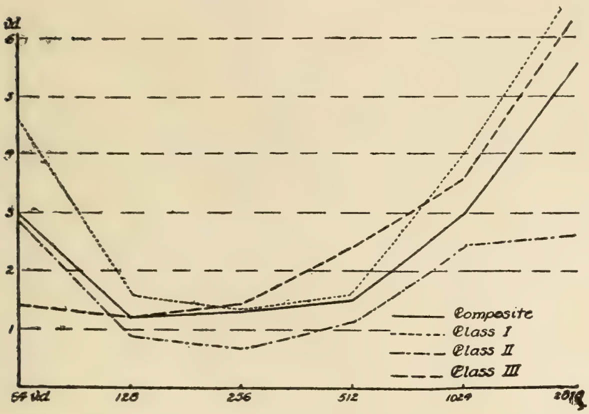

Fig. 7. The composite curves of the fifty observers and the three different classes.

at 256 than when it is at 128 v.d., and this is what occurs. With conditions such as they are, we are inclined to regard the results of this class of observers, at 64 v.d., as fairly accurate. The observers in question naturally do better work on the low tones. If such an interpretation is true, it would not be just to say that the curve of Class III is less consistent than the curve of Class II; each represents a different type and is consistent with itself throughout.

There are still to be considered the results of the thirteen observers which are not exactly comparable to any of the classes described above, because of certain irregularities occurring in their curves. It must be determined whether or not these ridges or elevations may be explained on the basis of daily fluctuations, in which case a sufficiently large number of observations would result in smooth curves, or whether the variations are due to natural weaknesses for the regions where they are found. The extent to which this latter explanation must be invoked, indicates an answer to the question of the frequency of gaps in the registers of individuals who have apparently normal hearing.

In Table I, the observers who have such curves are Nos. 4, 8, 11, 12, 18, 19, 25, 38, 40, 41, and 45. It will be noted, however, that at no place are the deviations from the normal very pronounced. All of them, excepting perhaps one, are doubtless due to certain factors which would have been eliminated by a large number of judgments. Daily variations, the relative amount of practice, and the accuracy with which the increments used corresponded to the true

thresholds, are the most important factors which have contributed toward the irregularities. With No. 5, for example, the experiment was begun at 256 v.d., so that the greater amount of practice would give the neighboring tones the advantage. With No. 11, the tone of 256 v.d. was first tried with a difference of 2 v.d., which was too large, resulting in an almost perfect series. Had the order in which these differences were presented been reversed, the threshold would probably have been very close to 1 v.d. Indeed, a number of these observers were given additional tests to determine whether or not these variations from the normal would hold. In each case as Table III will show, the curves became fairly smooth.

The curve of No. 4 is abnormal at 1024 v.d. in its relatively low threshold of 1.1 v.d. During an experimentation of one hour he made a record of eighty-eight per cent. on two hundred judgments with a difference of 2 v.d. But on the following day, a difference of 1 v.d. gave only fifty-two per cent. of the right cases. A larger number of observations would doubtless have resulted in a threshold more equal to that of the tone an octave lower.

But there is one observer, No. 18, who gives some evidence of a

TABLE III. Irregular results which additional observations have corrected

Observer	*64*	*128*	*256*	*512*	*1024*	*2048*
19	5.2	0.6	1.1	0.8	4.1	9.7
		0.6	0.6	1.1		
41	2.4	0.8	0.7	2.1	1.8	3.5
				1.8	3.5	
35	6.4	1.1	1.5	1.1	6.4	9.9
		1.8	2.2	2.6		
38	0.7	0.6	0.7	2.1	1.6	3.7
			1.1	.9	1.3	
12	3.7	0.7	1.5	2.4	4.3	3.5
				1.8	3.3	

slight weakness in the region of 256 v.d. His threshold at this point was derived from four hundred judgments. The first half of the number with a difference of 3 v.d. gave a threshold of 2.9 v.d., while the second half with a difference of 2 v.d. gave a threshold of 2.3 v.d. At no time was he able to approach a threshold of 1 v.d. On the other hand, with the tones above and below, he made low and consistent thresholds. It is difficult to account for this high threshold at 256 v.d.; the observer himself could offer nothing as a basis for explanation. The affective element, association and imagery, and inherent characteristics of volume and intensity may have played varying rôles in causing the discrepancy. At any rate the differ-

ences are not sufficiently great to be regarded as representing gaps.

We have found, then, from this study of the curves of discrimination of pitch of fifty normal observers no clear evidence of tonal gaps. The grosser irregularities which might arouse the suspicion of a gap are due to certain factors which have not been perfectly controlled. It is highly probable that with more extended observations the irregularities would have been eliminated. It must be kept in mind, however, that this conclusion has reference only to observers with apparently normal auditory capacity; with respect to individuals whose audition is unquestionably recognized as pathological, this study has nothing to offer.

Relation of Musical Training and Expression to Discrimination of Pitch.—The question naturally occurs in a study of this kind as to the nature and extent of the correlation between musical education and pitch-discrimination. It seemed obvious that if a correlation existed it would be between discrimination and musical expression rather than between discrimination and mere technical training. The Pearson method of rank difference was used to determine the correlation. The mean of the six levels in the range for each of thirty-eight observers gave a value for the ranking of the individuals according to their capacity for the discrimination of pitch. The records of the remaining twelve were not included as most of them were advanced students whose greater experience in work in the laboratory might possibly put them in a slightly better class, while with one or two others, information regarding their musical training was not at the time available. The ranking according to expression was not quite so simple. For this purpose an evaluation was made of the answers to the questionnaire, which was an exact duplicate of the one published by Professor Seashore in his Preliminary Report (13). To recall, there are three questions under the topic "Musical Expression": namely, (1) Favorite selections you can sing (by ear? by note?), (2) Favorite selections you can play (by ear? by note?), (3) Singing or playing in public (parts, occasions, etc.). The individuals were instructed to give as specific information as possible. But the comparison of the two functions showed no correlation whatever.

It was still believed, however, that there must be some difference between the discriminating capacity of those who seemed to be the

most musical and those who appeared to be the least, as far as previous experience was concerned. Again the questionnaire, to which reference has been made was resorted to, but this time the questions were designed to reveal the amount of training. They were as follows: (1) Musical training in public schools, (2) Private vocal lessons (when, where, how long, etc.), (3) Private instrumental lessons (when, where, how long, etc.). The observers were then equally grouped in two divisions, the first group consisting of the better ones in training and expression and the second of the poorer ones. The mean threshold for each group for the different levels is recorded below:

Table IV

V.d.	*64*	*126*	*256*	*512*	*1024*	*2048*
Group I	3.8	1.2	1.2	1.8	3.8	6.8
Group II	3.1	1.5	1.5	1.6	3.1	4.9

We find, then, that the group whose members have had greater musical education and more practice excel in capacity for discriminat-

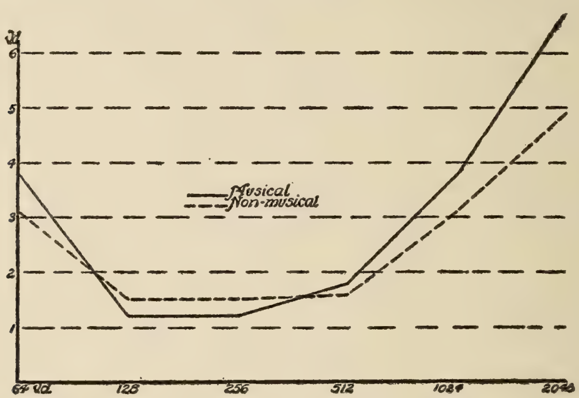

Fig. 8. Comparison of the musical and the non-musical.

ing pitch only at 128 and 256 v.d. But it is in this region that the above factors would have the most influence. Their effect upon the differential threshold of either extreme would be small because these tones are seldom used either in singing or playing. There is, then, some correlation between musical ability and discrimination of pitch in the central register. This is in general agreement with the conclusions of both Mount (6) and Smith (15) who found a fair degree of correlation between musical expression of pitch and dis-

crimination of pitch at 435 v.d. But we cannot agree with Stücker (17) in his assertion that musical observers, in general, show keener discrimination in the upper limit than the non-musical ones. Much depends upon the standard of classification for musical observers. Stücker may have reference only to observers of unusually fine ability in music. For such, his statement may be true, but for observers whose ability is not so exceptional it scarcely holds.

The frequency and distribution of the false judgments.—From the 39700 judgments it has been possible to determine definitely not only the frequency of the false judgments but also the way in which they have been distributed in the various levels. So large an amount of data should show whether or not there is a preference for one or the other order, and if so what relation this preference bears to sex, voice-register, and pitch.

In computing the number of errors, the results of sixty-two individuals, thirty-two women and thirty men, for at least three different tonal regions were available. The nature of the error in the wrong judgments at 64 v.d. was not recorded; the observer sat with closed eyes and gave oral judgments and the experimenter merely recorded the number of the errors. At the other levels, with one or two exceptions, the observer recorded *H* or *L* for each pair of tones. For 128, 256, 512, 1024, and 2048 v.d., then, the distribution of errors could be accurately studied. Just one-half of the sixty-two observers had a record for each of these five steps, for the other half discriminations were made at from three to four levels. Two different computations were therefore made, the first including the results of the entire number of observers and the second, only those which are complete for the five different levels. The total of the complete results could thus be used as a check upon the total of the incomplete results. Reference shall be made to the first, however, only in so far as it differs from the second.

TABLE V. Distribution of errors

Section 1

Computed from the results of sixty-two observers

A	B	C	D	E
128	8300	23.40	10.78	12.62
256	8600	25.13	13.31	11.82
512	10100	26.03	13.79	12.24
1024	8300	25.42	13.61	11.81
2048	4400	25.41	14.00	11.41
Total	39700	25.09	13.04	12.05

Section 2

Computed from the results of thirty men

128	4700	23.21	10.34	12.87
256	4800	25.00	12.83	12.17
512	6100	27.46	13.84	13.62
1024	4500	24.67	12.50	12.17
2048	2100	23.52	11.61	11.91
Total	22200	25.11	12.39	12.72

Section 3

Computed from the results of thirty-two women

128	3600	23.67	11.36	12.11
256	3800	25.29	13.92	11.37
512	4000	23.78	13.48	10.30
1024	3800	26.32	14.95	11.37
2048	2300	27.13	16.13	11.00
Total	17500	25.07	13.86	11.21

Table V is a record of the errors computed from the results of the total number of the observers. Column A represents the vibration-rate of the fork; B, the total number of judgments; C, the total percentage of error; D, the percentage of error when the second tone was lower; and E, the percentage of error when the second tone was higher. The greater number of judgments in the central register is due to the fact that irregularities occurring here necessitated further experimentation to determine whether they were due to subjective factors which were permanent or merely transient, or possibly to objective factors.

The final average of the percentage of right cases approaches to within .09 per cent. of the ideal of 75 per cent. When the different levels are considered collectively, the false judgments amount to 13.04 per cent. when the second tone is lower, and to 12.05 per cent. when the order of succession is reversed. There seems then to be a slight though not significant preference for the order in which the second tone is higher.

A difference is observable in the distribution of error at the various levels. At 128 v.d. more errors by 1.84 per cent. occur when the second tone is higher, but at the other levels there is a greater percentage of error with the opposite order. As shown in Table V the differences between Column D and E increase gradually from 256 to 2048 v.d. In the first computation, however, made from the complete results of a smaller number of observers, the order of second tone higher gives the smaller per cent. of error at each level. At 128 v.d., the difference in favor of this order is only .48 per cent., but

at 256 it amounts to 2.03 per cent., at 512. to 3.65 per cent., at 1024 to 2.68 per cent., and at 2048 v.d. to 2.46 per cent. On the average, then, judgments of difference in pitch are more accurate when the second tone is higher, *i.e.* given two successive tones of the same pitch, there is a slight tendency to hear the second as the higher, excepting at 128 v.d., where fewer errors are made when the reverse order is followed.

Difference of sex.—When the results are studied with respect to sex it is found that the above conclusion would not be valid for a group of individuals in which there was a much larger percentage of men than women.

In the study of differences of sex it is found that the women on the average, show a decided preference for the order in which the second tone is higher at every step except at 128 v.d., where the difference seems to be slightly in favor of the second tone lower. But it is at this latter level that the men show a very strong preference for the second tone lower, while in the other levels the difference in favor of either order is insignificant. This variation of sex affords additional evidence that normal illusions are greater with women than with men.

An arrangement of results according to voice registers of the observers brought out nothing new. The difference seems to be essentially between the voices of men and women. Had our observers been highly specialized singers, there might have been some difference showing itself in the different voice registers.

TABLE VI. Variation with sex

V.d.	*64*	*128*	*256*	*512*	*1024*	*2048*
20 women	3.2	1.2	1.4	1.7	3.8	6.6
16 men	2.7	1.1	1.0	1.6	2.4	4.8

The foregoing table and the accompanying figure show the results for the twenty women and the sixteen men who had a similar amount of training in experimental procedure. At first sight there seems to be a decided difference between the sexes, inasmuch as the thresholds for men are lower throughout the whole range than those for women. While there are differences in favor of the men, care must be taken not to attach too much significance to them. The differences at 1024 v.d., of 1.4 and at 2048 v.d. of 1.8, seem to be considerable, yet they are not much greater than should be expected when the total results are considered. At 256 v.d. the varia-

tion of .4 appears high when compared with the difference of .1 in the octave just above and just below. With such noticeable variation in the central region it is not so surprising to find much larger differences at the extremes where objective factors are not so well controlled. Smith (15) reports practically the same difference of sex as is shown in these results. He finds that at the ages of 17 to 20 and at maturity, the men surpass the women by an average of 0.3 v.d. at 435 v.d. It is evident that the men's curve presents a more satisfactory form than does that of the women, in that there is not so high a variation between the points of keenest discrimination and the

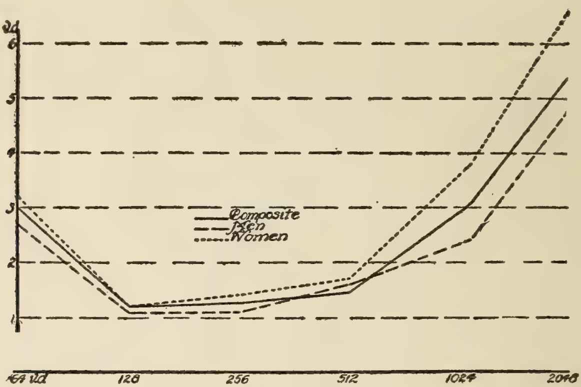

Fig. 9. The comparative curves of twenty women and sixteen men together with the composite of the fifty observers. (Table VI).

extremes. It may be that the cause of this arises from a possible inherent difference between the sexes in the method of meeting new situations. Or it may be that the men adapt themselves more quickly to experimental conditions and for this reason it has been easier to reach their physiological threshold.

Stücker (17) contends that the greatest sensitiveness to small differences of pitch lies with tenors and sopranos in the lower half of their voice registers, but with singers of bass and of alto parts, as a rule, in the upper half. In other words, the differences is not between the voices of men and the voices of women but between the relative height and depth of the voice register of both sexes. As none of my observers could be classed as professional singers, the results have little to offer either positively or negatively, in regard to Stücker's statement. Table VII indicates that only the results of

the soprano singers can be harmonized with the conclusion of Stücker. The finest sensitivity of the tenors is in the central part of their register and not in the lower, as he finds it to be; the basses made the best record in the lower part of their register, rather than in the upper; the baritones have done better in their register; and finally, the altos do better in the lower register and not in the upper. But these facts are not necessarily contradictory to Stücker's, inasmuch as the observers in this experiment represent only average ability as singers. One should have plotted the curves of a relatively large number of highly practiced singers before he would be able to add a conclusive word in answer to the problem which Stücker has suggested.

TABLE VII. Average thresholds classified according to voice register

Soprano	(16)	3.8	1.5	1.6	2.1	4.1	7.1
Tenor	(4)	4.1	1.5	1.2	1.2	2.3	4.3
Baritone	(15)	3.2	1.4	1.3	2.1	2.4	4.7
Alto	(8)	3.5	1.5	1.2	1.3	4.3	6.0
Bass	(7)	2.5	1.1	1.3	1.6	2.8	4.3

SUMMARY

(1) For individuals selected because of a slight superiority at 435 v.d., the composite absolute curve of pitch-discrimination within the limits of 64 and 2048 v.d. shows the keenest discrimination at 128 and 256 v.d. On either side of this central register, there is a rise in the curve which is relatively abrupt toward the lower limit but much more gradual toward the higher extreme.

(2) The relative curve takes the form of a continual decline from the lower to the higher limit. From 64 to 128 v.d. the decline is comparatively steep, but from 128 to 2048 v.d., it is very gradual, approaching approximately a horizontal line in the upper half of the register.

(3) Individual differences, factors which lead to confusion and to identification, and variation in practice and in attention are the principal conditions upon which the form of the curve depends. The variations in the curves of the different investigators are explainable on the basis of the varying degrees of influence of these conditions.

(4) Most of the individual curves conform more or less closely to one of the following types of curves; namely, (a) a curve in

which there is a relatively low value at some point in the central register and relatively high values at the extremes, (b) a curve in which the thresholds are fairly uniform throughout the entire range, and (c) one in which the threshold for 64 is considerably less than for 2048 v.d.

(5) There is very little evidence of tonal gaps. The grosser irregularities in a few curves, which at first seemed to indicate the presence of a gap, disappeared with more extended observations.

(6) A correlation between musical ability and discrimination of pitch occurs only in the central register.

(7) The women make more accurate judgments when the second tone is higher; their preference for this order increases in direct proportion to the pitch, within limits, excepting at 128 v.d. where the reverse order is slightly preferred. The men make fewer mistakes at 128 v.d. when the second tone is lower, but at the other levels no particular preference for either order of succession is observable.

(8) The men surpass the women in discrimination of pitch at every level in the register; this variation between the sexes is the greatest at the extremes.

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THE DURATION OF TONES, THE TIME INTERVAL, THE DIRECTION OF SOUND, DARKNESS AND QUIET, AND THE ORDER OF STIMULI IN PITCH DISCRIMINATION

BY

DAVID ALLEN ANDERSON

I. Most favorable duration of the tones

In this investigation to ascertain the relative favorableness of different durations of tone in pitch discrimination, the tones were produced by tuning forks from the "standard pitch discrimination set" as described by Professor Seashore (1) reenforced by Koenig adjustable resonators suspended behind a revolving slit-disc which was driven by a synchronous motor (2,3).

The tuning forks were tuned to an accuracy of ± .015 v.d. They were held firmly by the fingers near the end of the stem and energized by striking the middle of the prong lightly against a sounder made of ¾ in. lead pipe covered with a soft rubber tubing and resting on a leather cushion filled with sand. When they had been set in motion the forks were held directly in front of the mouths of the resonators during the passage of the open slits in the intervening revolving disc. Revolving discs made from cardboard, in which were slits cut in appropriate sectors, regulated the duration of tones and the interval between them. The disc proper prevented the passage of the vibrations from the forks to the resonators while the slits admitted of their free passage. The length of the slit determined the duration of the tone and the size of the sector between governed the length of the interval. When a slit passed a fork the resonator would take up the vibrations. The result was a clear and pure tone, clean cut at beginning and end. The intensity was kept as regular as possible without maintaining an identifiable uniformity. An effort was made to change the forks from hand to hand and to govern the duration of time between the energizing of the forks and the hearing of the tones in such a way that the observers could get no clue regarding the order in which the tones were to be given. Whether the higher or lower tone was to be given last was regulated by a key prepared beforehand according to chance, except that not more than three consecutive cases of one kind were allowed.

The two resonators were fastened side by side immediately behind the revolving disc.

The speed of the synchronous motor used in driving the revolving discs is controlled by a tuning fork and gives an accuracy in the time element far beyond the requirements of this experiment.

A time interval of ⅛ second was chosen arbitrarily and was kept constant throughout. Ten judgments constituted a group and ten groups or columns (100 trials) confined to one duration made up a set. When a group of ten judgments had been taken and recorded in a column, another group followed, and so on until the set was completed. When one set had been given it was followed by a set of another duration and so on throughout the series. Generally about four sets were given at a sitting.

The observers were Professor C. E. Seashore, and three graduate students, namely, G. H. Mount, L. E. Widen and W. R. Miles; all of whom were at the time pursuing experimental problems in the laboratory and had quite extended experience in observing tones. Each of them had a threshold of 1 v.d. or less on the basis of 75 per cent. correct judgments; hence 1 v.d. was used as the pitch interval throughout the tests. The observers were permitted to choose a location in the room which seemed favorable, and comfortable with the understanding that it was to be kept unchanged.[1] They listened to the two tones, judged the latter as higher or lower than the former, and recorded the decision (H for higher and L for lower).

TABLE I. Effect of differences in duration of tones
(Time interval ⅛ second; pitch interval 1 v.d.)

	S.			*Mo.*			*W.*			*Mi.*			*Ave.*
Duration	%	m.v.	n.	%	m.v.	n.	%	m.v.	n.	%	m.v.	n.	
⅛ sec.							72	2	500				71.6
¼ sec.	58	5	500	73	9	500	76	4	500	67	2	500	68.4
⅜ sec.	68	6	500	81	5	500	78	3	500	76	5	500	76.0
½ sec.	75	3	500	84	4	500	76	3	700	74	4	500	77.5
1 sec.	76	8	500	82	4	500	79	3	500	84	4	500	80.4
2 sec.	87	5	300							89	2	300	88.3
1st tone 2, 2nd, ½ sec.	77	1	200							86	2	200	82.0

%, per cent. of right cases; *n*, number of trials; *m.v.* mean variation for successive hundreds of trials.

[1] Two observers each made one change in position but their records in both positions were so distributed throughout the series as not to interfere with the results. The records of both were materially improved by the shift. The influence of position of the observer with reference to the origin of the tone is discussed later.

Table I shows a general tendency in favor of the shorter durations. There is practical uniformity in this general tendency among the several observers, there being but three steps that are exceptions: Mo.'s average at ½ second is higher than his average at 1 second, and W and Mi. make higher averages with a ⅜ second duration than when it is ½ second in length. However, the increase in ability with increase in duration of tone is comparatively small.

Introspections indicate that the most favorable feeling attending the hearing of tones of a certain duration may or may not parallel the percentage of correct judgments. It is also noted that when tones are of any long duration, judgment is usually made as soon as the essential character of the second tone is perceived without waiting for its cessation. Some tests were thereupon made on S and Mi. using durations of 2 seconds and ½ second in each couplet. This experiment consisted of two hundred judgments by each of the observers and resulted in a general average of 82 per cent. correct judgments. (See last line of Table I.). There seems to be therefore no advantage in making the second tone more than ½ second in duration.

Taking all things into consideration, it appears that the initial tone should have a duration of about 1 second, while the second tone need not exceed ½ second in duration. The demands for economy justify these limits even though longer intervals result in a slight increase in efficiency.

II. Most favorable time-interval between tones

The problem was to determine whether the time-interval between tones should be of a definite length and, if so, what it should be. In this problem the laboratory conditions, dates of experimenting, observers, apparatus, and methods were the same as in the preceding section. The discs were made so as to vary the interval and keep the duration uniform. The time intervals tested were 0, 1/16, ⅛, ¼, ½, 1, 2, 3, and 4 seconds. The duration of tones used was kept constant at ½ second.

Upon examination of Table II we notice that the gross averages for the several intervals are fairly uniform while the records of each individual vary widely at each step in the series. For this group the intervals tested are practically equally favorable, with possibly a slight tendency in favor of the shorter. Introspections

TABLE II. Effect of difference in time interval between tones

(Duration ½ sec.; pitch interval 1 v.d.)

Interval in seconds	*S.* %	m.v.	n.	*Mo.* %	m.v.	n.	*W.* %	m.v.	n.	*Mi.* %	m.v.	n.	Ave.
1/16 sec.	78	9	300				95	1	300	87	4	300	86.6
1/16 sec.	73		100	80	5	500	87	8	500	89	7	500	84.6
⅛ sec.	68	10	200	74	7	500	87	7	500	92	1	500	82.5
¼ sec.	63		100	81	5	500	87	6	500	90	2	500	84.7
½ sec.	61		100	78	5	500	87	6	500	91	4	500	83.4
1 sec.	74		100	75	5	500	83	7	500	84	7	500	80.4
2 sec.	67	4	200							86	1	200	76.7
3 sec.	71		100							80		100	80.5
4 sec.	78		100							78		100	78

%, per cent. of right cases; *n*, number of trials; *m.v.* mean variation for successive hundreds of trials.

indicate that conditions of stress, strain, annoyance and fatigue are experienced when long intervals are used. It is advisable to economize time and energy by adopting a short interval or by excluding it altogether.

The results here reported are in accord with, and supplement in so far as they cover the same ground, the studies of Wolf (4), Angell and Harwood (5), and Whipple (6), though these investigators were concerned chiefly with the influence of the time interval upon tone memory and imagery. The shortest interval tested by any of these experimenters was one second and comparatively few records were taken where they used an interval of less than two seconds. Their results are fairly uniform in showing the advantage in a short interval.

III. Most favorable direction of the sound

The problem was to ascertain whether any one direction of the source of the sound was most favorable in discrimination of pitch. The experiments were performed in July 1911 in the open air away from every influence of limiting walls or surfaces aside from the ground and the grass. The sounds were produced by tuning forks with resonators.

The observers were Professor Seashore, Jessica E. Strawbridge, T. F. Vance and E. T. Walker. The three mentioned were chosen from among the graduate students in the University of Iowa. All but one of the observers were practiced in pitch discrimination. A pitch interval of 1 v.d. was used with *S* and *St.* and 2 v.d. with *V* and *Wa.*

In the experimenting the observers were seated about ten feet from the source of the tone. Beginning in a position directly facing the source of the stimulus the observers turned to the left ninety degrees at a time so as to hear the tones exactly from the front, the right, the back and the left in successive series of trials. Making twenty judgments in each position, they continued to turn until one hundred judgments were made from each of the four directions. Table III gives the results in percentage of correct judgments of each individual in each of the four positions and the averages for the group, showing that in the open air there seems to be no significant effect of direction of the sound upon ability in pitch discrimination.

TABLE III. Effect of direction of sound

	S.	*St.*	*V.*	*Wa.*	*Ave.*
Front	71	64	56	72	65.7
Right	66	55	58	72	62.7
Back	64	53	63	69	62.2
Left	76	62	62	69	62.2

A similar set of experiments was then tried in a room 15 x 18 feet square with the observer in a selected series of positions with reference to direction of sound, relation to walls, and distance. On the whole the same conclusion was reached as for out-of-doors. However, strong individual preferences were expressed. There was no clear evidence of correlation between feeling of favorableness and actual ability.

IV. Darkness and quiet

The experiments herein reported were designed to ascertain the influence of occupying the "dark room" upon accuracy in judgments in pitch discrimination. The work was done in July 1911 in room No. 210. L. A. which is a "measurement room", (15 x 18 x 13 feet, well lighted and occupied by apparatus and laboratory furniture, quite resonant), and in the light, sound, and jar proof room described in Vol. III of these Studies (7).

The tuning forks with resonators were used as before, without the discs. The observers were Professor Seashore and graduate students F. O. Smith and E. T. Walker. The position of the observers with reference to the origin of the tones was kept uniform in the two rooms throughout the experimenting. With *S* a pitch-interval of 1 v.d. was used, with *Sm.* and *Wa.* 2 v.d. *Wa.* was an unpracticed observer.

Each observer made two hundred judgments in each of the two rooms. One hundred in the "dark room" were followed immediately by the same number in "the measurement room".

TABLE IV. Effect of darkness and quiet

	S.	*Sm.*	*Wa.*	*Ave.*
"Dark Room"	80	54	64	
	73	55	81	67.8
"Measurement Room"	75	51	56	
	75	62	75	65.7

The results indicate that accuracy in judgment is about equally favored in the two rooms. Introspection shows that while the silence and freedom from distraction of the dark room are soothing, they also make the observer more critical about the stimulus. As one observer said, "Any irregularity in the tones was extremely annoying in the dark room, but in room 210 the accessory sounds made the stimulus seem smooth and soft."

In view of these results, including introspections and observations, it appears that accuracy in judgment of pitch of clearly audible tones will be as high in an ordinary laboratory-room as in a quiet room. The freedom from distractions in the dark and quiet room has a soothing effect upon the observer but, owing to the absence of distracting influences, the observer in the quiet room detects minor qualities and characteristics of tones and methods of procedure which would not come to consciousness in the ordinary laboratory. The normal noises and lights of an ordinary room seem to soften and smooth the stimulus.

V. The order of stimuli

During the progress of the experiments reported in the preceding sections one observer noted that his first judgment in a column (group of ten made in rapid succession) nearly always designated the second tone as higher and that such judgment was frequently in error. In order to ascertain whether or not this was a general tendency, the experimenter examined his records of more than 15000 judgments made by eight observers and written down in 1517 columns of ten judgments each.[2] The recording of judgments in successive columns corresponded to the method of presenting tones in successive groups of ten trials each. The trials in the individual

[2] A few columns include twenty judgments each.

groups followed each other in rapid succession, but there was a slight pause between groups. Therefore the first judgment in each of the 1517 columns or groups is the only one now under consideration. Table V shows the record made by each of the eight observers.

TABLE V. Effect of order of high and low

Observers	Number of judgments	Percentage of errors when judging High	Low
S.	362	77	23
Sm.	20	86	12
St.	20	100	0
Mo.	450	50	50
W.	310	31	69
Mi.	295	64	36
Wa.	40	57	43
V.	20	33	67

Now by actual count of all errors made in the list of over 15000 judgments, there is found no important constant tendency, 50.8 per cent. of the errors falling on the side of low. Without any constant tendency the errors should be about equally distributed between high and low in Table V. But such is not the case with observers *S., Mo., W.* and *Mi.,* for whom the record is extensive; *S.* and *Mi.* tend to judge "high", *W.* to judge "low" and *Mo.* happens to be exactly neutral. For the other observers the records are too few to be of much significance. We must therefore conclude that there are probably fixed individual tendencies to judge high or low but there is no constant group tendency. The order of tones therefore remains a factor which must be kept under control in experimenting.

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THE EFFECT OF INTENSITY AND ORDER ON THE APPARENT PITCH OF TONES IN THE MIDDLE RANGE

BY

ROLLAND M. STEWART

In the study of various phases of pitch discrimination in this laboratory the problem of controlling the intensity of sound has proved persistent. The writer therefore undertook to ascertain some of the principal features of this tendency. The work was begun with two differential forks, standard 128 v.d., mounted on tripods and energized electrically. Before these, a Helmholtz resonator was suspended which could be swung freely into position in front of either fork. The intensity was judged subjectively by the experimenter and was varied by the distance of the resonator from a point of greatest efficiency in front of the fork. After some preliminary work this apparatus was abandoned on account of the disturbance coming from the interruption of the current. The forks were remounted on handles easy of manipulation and were struck on a sounder and presented alternately in front of the same resonator but these forks proved to be unwieldy and it was found practically impossible to avoid the error of identification. They were therefore abandoned again and it was decided to attempt the test with a set of standard forks at 435 v.d.[1]

The real difficulty with 128 v.d. forks lay in the fact that one fork was tuned by sliding weights. The final test was therefore made with a series of differential forks, standard 435, which were tuned permanently to given fixed increments in the making and had no adjustable parts. The forks were presented to the resonator at a uniform rate according to a pre-determined scheme of distribution of weak and strong sounds according to prepared keys. The keys used were known only to the experimenter and were prepared by determining the order of the weak and strong sounds by chance

[1] This incident has proved in the light of more recent work to be of far greater importance than was known at the time, since, as is shown in the accompanying article by Miss Hancock, the law of intensity effect is quite different at 128 v.d. from what it is at 435 v.d.

except that a sufficient number of substitutions were made to get the same number of trials of each kind.

Two keys were used. Key I called for 150 observations: 50 cases of the same pitch and varied intensity; 50 cases with a pitch increment and constant intensity; and 50 cases with a pitch increment and varied intensity—thus high and strong, high and weak, low and strong, low and weak.

Key II called for 192 observations, 32 of which were introduced to exclude or to determine a measure of the so-called time error, *i.e.*, a tendency to call a tone either high or low simply because it is the second tone of a pair. For these trials the same fork was sounded with the constant intensity in both presentations. The remaining 160 observations were divided equally among the four variations mentioned in Key II; namely, high strong, high weak, low strong, low weak.

All observations were made in the light, sound, and jar proof room. Trained observers were used for the most part. Three of these, *D, F* and *H,* had engaged in extensive practice in the study of the problem of pitch discrimination. Four, *A, C, E* and *G,* were working in related problems at this time. Only one observer, *B,* was inexperienced in this problem, but she proved to have a fine ear for pitch discrimination where the intensity was constant.

TABLE I. The effect of the intensity and the order of the tones

	Key I		Key II			
Obs.	Intensity Constant	Intensity Varied	Intensity Constant	Intensity Varied	Weak	Strong
A	77	76	90	86	83 L	67 H
B	95	76	96	81	74 H	70 H
C	80	80	100	77	76 H	57 L
D	75	74	73	76	61 L	63 L
E	75	60	100	66	89 H	96 L
F	63	68	90	80	60 L	58 H
G	84	71	90	57	67 L	67 L
H	77	62	100	87	67 H	52 H
Ave.	78	71	92	76		

In all about 7500 observations were taken as a basis for these records. Table I shows the effect of the variation of intensity on the judgment of pitch. The records are given separately for Key I and Key II for each observer; and, in the last two columns of the table the same records for the two keys are combined and distributed with reference to weak and strong. In the former the

numbers indicate the per cent. of right cases under the four conditions named and, in the latter, what per cent. of the weak and strong respectively were called high (H) or low (L) when actually equal in pitch.

Examination of these figures shows that the varying of the intensity causes confusion which results in a poorer record than when the intensity is constant. This is true in the records both under Key I and Key II. The ratio of the right cases for "intensity constant" as compared with "intensity varied" is on the average 78: 71 in Key I, and this tendency is true of all individuals except one (F) for Key I. For Key II the corresponding ratio of the per cent. of the right cases is 92: 76.

The analysis of the results with reference to weak and strong proves that, for these observers, there is no constant tendency to identify weak or strong and high or low. However, examination of the individual records shows that this is not due to the absence of constant tendency in individuals but rather to the balancing of opposite tendencies in different individuals of the group. The percentages in the table show these individual tendencies to be quite marked. On the whole four (A, B, D and F) have a constant tendency to call the weak low and the strong high; while the other four (C, E, G and H) show the reverse tendency.

Table II contains the distribution of the same records, showing other details in regard to the intensity-pitch illusion. The notation of the table is self-explanatory; the abbreviations HS, HW,

TABLE II. Redistribution of same data as in Table I

Key I	Observers	A	B	C	D	E	F	G	H	Av.
Intensity	High called low	25	6	11	37	25	38	11	32	23
Constant	Low called high	26	4	31	13	25	35	23	11	21
Key I	HS called low	11	2	27	34	73	23	16	50	29
Intensity	HW called low	21	44	12	20	12	33	17	17	22
Varied	LS called high	43	31	21	24	7	43	11	36	27
	LW called high	18	12	59	22	73	25	59	50	40
Key II	HS called low	5	4	27	15	55	15	62	7	24
Intensity	HW called low	22	47	5	27	22	32	15	12	23
Varied	LS called high	20	23	5	40	22	22	25	15	22
	LW called high	7	1	45	15	35	10	67	20	24
	Tendency of second tone	53H	74L	50	66H	69H	81L	62H	53H	

HS, HW, LS and LW stand for High Strong, High Weak, Low Strong, and Low Weak respectively.

LS, LW, standing respectively for high strong, high weak, low strong, low weak. A comparison of the strength of the illusion for each individual in the second and third horizontal sections of this table with the distribution of errors in the first horizontal section (for "intensity constant") shows that these personal equations are sufficiently large to be recognized as fairly prominent individual tendencies that must be taken into account in any comparison of the pitch of two tones.

The above named conclusions are fully substantiated by preliminary observations on 22 observers whose records are not included in the above table because they were taken under somewhat varying conditions. Of these 22 observers 9 called the weak high and 10 low; 10 called the strong low and 12 the strong high. Although the introspections were studied quite carefully, no satisfactory explanation could be found to show why these errors occur. It was first thought that the primary tendency was to identify strong with high and to assume that when the opposite tendency appeared this was due to a reaction, conscious or unconscious, as a correction to this tendency which the observer might expect in himself. But this interpretation is probably not true since it is shown in the article referred to above that for low tones the tendency is just the reverse. We are therefore left without any satisfactory interpretation of the phenomenon and with the impossibility of knowing what direction the illusion will take in a given individual.

The order of the sound was so distributed as to eliminate that source of error for the main purpose of this experiment. The bottom section of Table II shows that there is no constant tendency in the time-error for these observers as a group: there is about as strong tendency to call the second tone low as to call it high. It is, however, clear that quite marked individual tendencies exist as in the 74 per cent. low for *B,* the 81 per cent. low for *F,* the 66 per cent. high for *D* and the 69 per cent. high for *E.* The conclusions on this point in the foregoing article by Anderson (Section V) are thus sustained, both as to the divergence in the direction of the tendency and the characteristic magnitude of the error.

THE EFFECT OF THE INTENSITY OF SOUND UPON THE PITCH OF LOW TONES

BY

CLARA HANCOCK

In pitch discrimination tests, a difference of intensity in the sounds compared has proved so important a source of error as to require investigation. A series of experiments were conducted by Stewart, as reported in the foregoing article, (1), with forks of 435 v.d. for the purpose of discovering the effect of intensity variation. It was on his tests that Professor Seashore based the following statements in his preliminary report on, "The Measurement of Pitch Discrimination." (2).

"Extensive experiments show (1) that both trained and untrained observers may be influenced by intensity in their pitch judgment; (2) that, although there is a tendency among the untrained, especially the ignorant, to judge the loud tone the higher, it may work either way; (3) that the same individual may show one tendency at one time and the reverse at another; (4) that for trained observers the two tendencies are almost equal; and (5) that the tendency is more serious for large than for small intensity differences."

Later, during a series of experiments on accuracy in singing the tones of forks of from 109 to 308 v.d., Miles (3) found results that differed materially from those of Stewart. He found that an increase in intensity of the standard tone regularly caused a lowering in the pitch of the reproduction when that was of medium intensity, and that when the standard tone was presented with medium intensity, if it was reproduced loudly, it was sharped. This indicated that the effect of intensity on pitch discrimination might not be the same for low tones as for high ones, and that the conclusions of Stewart's experiments might not be applicable to tones of low pitch.

The following series of experiments were undertaken to determine whether or not, in the sounds of tuning forks of 128 v.d., a difference of intensity produces any more constant illusion as to pitch than in the higher tones.

A set of forks was tuned from 128 v.d. upward with intervals of 1, 2, 3, 5, 8, 12, 17, 23, 30, 38 and 47 v.d. Later it was found necessary to tune a few lower than the standard for corresponding increments. The range is from 123 to 175 v.d. In the first series of tests no resonators were used. The difference in intensity was controlled by the force with which the forks were struck on the sounder and by the distance they were held from the ear. The aim was to have the faint sound just loud enough to be distinctly heard as a tone, and the loud one as loud as possible without interfering with its quality as a tone. The method of limits was used. The standard, 128 v.d., was always made faint, and the variables loud. The sounds were presented in two orders in alternating series: (1) a series with the standard first, followed by the variable; and, (2) a series with the variable first, followed by the standard. With the last observers several series were given also with no difference in intensity, for the purpose of comparison. In these the order was variable.

The point in each series at which the observer's judgment changed from "high" to "low", or "low" to "high" was taken as the tone which, when loud, was perceived as equal to the standard 128 v.d. when faint. Many of the observers' judgments changed several times through a range of several vibrations, and in these cases the mean between the highest and the lowest change of judgments was used. As the range of uncertain judgments varied considerably among the different observers, that, as well as the amount of the error, is stated in the tables.

The results of this series of tests are shown in Table Ia. The average error made in comparing the loud sounds with the faint ones, and the average range through which the judgments were not constant or certain are indicated for each observer. When the faint sound (128 v.d.) was given first, a loud sound actually several vibrations higher was selected as of the same pitch. When the loud sound was given first, in five cases the average error was zero, and in one case a sound 1 v.d. lower than the standard was selected; in the case of the thirteen other observers the error was in the same direction as before. In both series, the average of the errors is 6 v.d. Most of the observers found it more difficult to judge when the faint sound was second than when it was first, and there is greater variation among the judgments.

TABLE I.

(a)

	Intensity Equal		Intensity Different			
			Standard first		Standard second	
Obs.	Error	(Range)	Error	(Range)	Error	(Range)
Mc.G	0	(3)	3	(9)	2	(7)
Go.	1	(3)	2	(6)	0	(2)
Ch.			6	(0)	7	(0)
Cu.	2	(5)	6	(5)	9	(8)
Bo.			9	(1)	7	(5)
Ar.			14	(4)	9	(2)
Ge.			13	(4)	11	(5)
On.			7	(12)	29	(10)
Va.			5	(3)	8	(3)
Th.			3	(1)	0	(2)
Sa.			8	(1)	6	(1)
Le.			1	(4)	1	(2)
Ba.			13	(8)	18	(8)
Pi.	0	(2)	4	(2)	2	(3)
Ma.	0	(2)	2	(0)	0	(1)
Gr.	0	(1)	4	(2)	0	(5)
So.	1	(3)	13	(7)	9	(5)
Dm.	0	(5)	8	(3)	1	(5)
Li.	0	(1)	5	(1)	0	(1)
S.			8	(0)	3	(0)

(b)

Li.	0	(3)	1	(7)	0	(1.5)
Cu.	0	(4)	3	(6)	4	(8)
Ma.	0	(0)	4	(0)	2	(0)
Mc.G.	0	(0)	4	(2)	2	(3)
Ge.	2	(2)	7	(0)	7	(0)
So.			9	(1)	9	(1)

(c)

Cu.	5
Ge.	1.5
Ma.	2
So.	3

There seemed to be a possibility that the disturbance caused by having the loud sound close to the ear affected the judgment of its pitch, and for that reason further tests under other conditions were given to several observers. The same forks were used, but a glass funnel was held to the ear to prevent any effect on the sound that might result from the shape of the ear lobe. The results are shown in Section b. While the error in judgment was considerably reduced for several observers, it was still in the same direction as before.

Further tests were given, using a resonator with the forks, so placed that the sound was in the median plane instead of close to

one ear. The method was changed in this series. Pairs of forks were selected of different intervals from .5 v.d. to 8 v.d. according to the judgments of the observer. Small intervals were used first, and these were increased until the judgments were given correctly and with certainty. In these tests, the observer knew that the same pair of forks was being used for several times in succession, the weak one being sometimes first and sometimes second; in this way he had an opportunity to verify or contradict a judgment in the following one. He did not know what forks were used, nor the results of the former experiments. The interval at which the judgments were "equal" or were about equally divided between "high" and "low", was taken as the amount of the observer's error. This varied from 1.5 to 5 v.d., and was in the same direction as the previous results. Further, to test the fact of the existence of the illusion, the same fork was sounded strong and weak; and also the pairs of forks used before were reversed, the lower being sounded louder. With the combinations reversed, the judgments were always correct, even with as small an interval as .5 v.d., indicating an apparent increase of the interval; when the same fork was used for the two sounds, the louder was judged to be low, except that one observer, Ma., in the case of one fork, called the sounds equal in pitch. Ma. also reported an apparent rise in the pitch of the loud fork as it was taken from the resonator.

These tests show that the illusion of the earlier tests was not due to a disturbance at the ear. A repetition of the test with one observer, Cu., in which the last method was used, but without the resonator, shows an illusion of 6 v.d., about the same as that in the first series of tests.

Both experienced and inexperienced observers were used, some with special ability and some without. There seems to be no correlation between the amount of the error and either practice or musical ability. Their only effect is to make the judgments more constant.

The illusion of pitch which is due to strength of tone is then clearly established for relatively low tones and found to be different from that of tones of the middle range. But what is the situation for high tones? Preliminary tests were made of eleven observers with 512 v.d. forks. They show, on the part of six observers, a decided illusion in the same direction as with 128 v.d. forks; one, Ma. (as in the 128 v.d. test) reported a change in pitch in the

same direction when the fork was brought to the resonator and removed from it. With three others there appears to be no illusion, and with one it is reversed, the strong tone being called higher. Similar tests on thirteen observers at 1024 v.d. show the same direction of the illusion as low tones in one case, a reversal of it in three cases, and no illusion in the other nine cases.

The conclusions of this investigation may therefore be summed up as follows:

(1). With forks of 128 v.d., a difference of intensity causes an illusion in pitch which is constant in direction, though variable in amount, a louder sound being judged lower than a faint one of the same pitch. The magnitude of the error seems to increase with increasing strength of tone, the average illusion for the differences in strength here used being about 6 v.d. Many observers show considerable confusion on account of the difficulty of comparing tones of different intensity. There is no constant relation between this confusion and the amount of the illusion. Musical ability, and experience seem to lessen the confusion, but not the amount of the error.

(2). At 512 v.d. the tendencies seem to be approximately as Stewart found them at 435 v.d., with a somewhat stronger tendency to judge the loud tone the lower. At 1024 v.d. the effect of intensity difference seems to be less disturbing than in lower tones.

(3). In general, difference in the intensity of tone is always a disturbing factor in pitch: the illusion is strong and constant in direction for relatively low tones; with rising pitch, it decreases and may vary in direction.

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THE MEASUREMENT OF TIME-SENSE AS AN ELEMENT IN THE SENSE OF RHYTHM

BY

FELIX BRUENE ROSS

The object of this series of experiments was to make an accurate measurement of the time sense as an element in the sense of rhythm, primarily for the purpose of standardizing a test for the measurement of individual differences.

It was necessary to procure some means of producing regularly recurrent sound stimuli and of varying the interval under control. Hitherto kymographs have been mostly used for the running of some sort of "time-sense apparatus", such *e.g.,* as that of Meumann. But it can be easily shown that a clock-work is not sufficiently accurate for the finer measurements of this kind.

We are fortunate in having in the laboratory a synchronous motor which very well fills the needs of this experiment. This motor has been described by Lorenz (1) and Seashore (2).

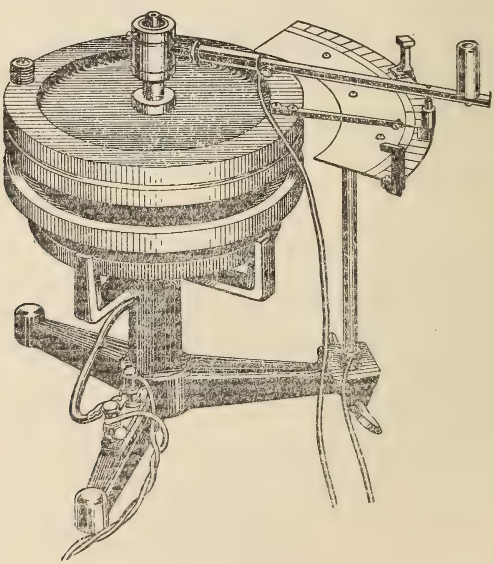

Fig. 1. The horizontal type of synchronous motor with rhythm attachment.

Fig. 1. shows the motor with the accessaries as here used. The top wheel is a balance wheel set on the axle of the motor proper. The pointer attached to this wheel makes contact with the knife-edge on the long movable and insulated arm. This arm may be set by hand for any point on the scale. To secure accuracy the two clamps on the scale plate are used as stops.

A telephone receiver was placed in the circuit of the above make-and-break contrivance in circuit with a battery. The momentary make-and-break of the current produced a distinctly audible and clear click which was used as the stimulus.

The balance wheel carrying the contact arm revolved clockwise at the rate of one revolution per second. If the long arm were held stationary the time interval would be constant,—one second. To change the interval it was only necessary to swing the arm through the required distance as indicated on the scale.

The motor with all the above mentioned accessaries was tested for accuracy by the spark method of recording and it was found that the limit of accuracy for the apparatus as thus operated is .0008 seconds.

The motor was located in a distant room and the telephone receiver connected with it suspended eight feet from the floor in the center of the room in which the measurement was made. A few preliminary trials were given in order that the observer, or observers, as the case might be, might have a clear understanding of the nature of the experiment. This done, a signal was given and the test began. A single test consisted in sounding ten clicks in succession with the understanding that the first five marked equal intervals but that in the last five there would be one short interval: and it was the task of the observer to detect this one. Seven different steps of change were used in successive groups of trials: namely, .02, .03, .05, .08, .12, .17 and .23 seconds. Twenty tests were given on each increment beginning with the largest and taking them in order. The right and wrong cases were counted and the records checked accordingly. The amount of deviation which would yield 75 per cent. correct judgments was computed, using only records between 65 and 90 per cent. right judgments. The average of the thresholds thus computed was taken for as many of the above steps as yielded records within the limits of 65 and 90 per cent.

The results here reported are based on four group measurements and a series of individual measurements. The group measurements were made in a large room into which noises from the halls and streets penetrated freely and proved a disturbing element. In the first three group measurements such disturbances as are characteristic of a large group were present. But in taking the fourth group measurement these objectionable features were eliminated by limiting the observers to a small number in this group. All individual measurements were taken in the dark-and-quiet room of the laboratory.

The results of the first group measurement taken on 200 observers, mostly sophomores, in two divisions of 100 each, are shown in Fig. 2. Those of the second and third, which were taken on 256

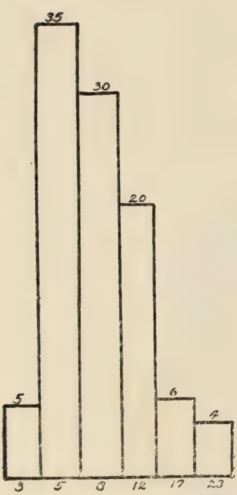

Fig. 2.

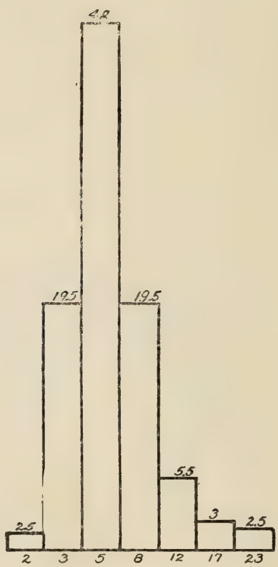

Fig. 3.

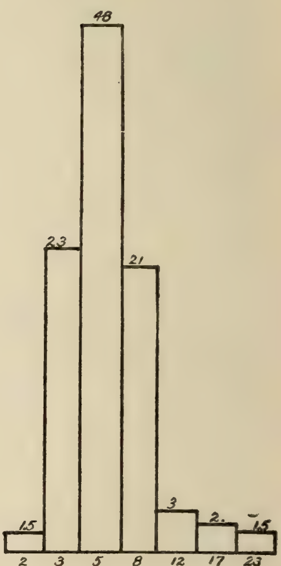

Fig. 4.

observers each, mostly sophomores, in two divisions each, with approximately an equal number in each division, are shown in Figs. 3 and 4 respectively. The distributions for these large groups of one hundred or more are all similar. This would indicate that this distribution is characteristic for different groups and for the same group at different times.

The third group measurement is a repetition of the second taken on the same persons as accurately as possible, and by the same method and under similar conditions, except those of practice, the object being to determine to what extent the distribution for a group is stable—the coefficient of correlation was computed and was found to be .69, P. E. r .027. This is a fairly satisfactory correlation considering that there are factors in the test not yet under control.

In order to determine the effect of the disturbances due to large groups, the same test was made on a group of thirteen in a small and relatively quiet class room. The result is shown in Fig. 5a.

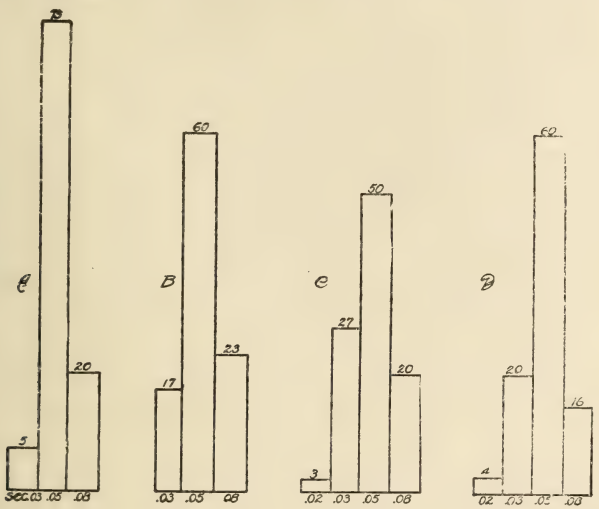

Fig. 5a. Fig. 5b. Fig. 5c. Fig. 5d.

Here the mode is the same (.05) as before but the extremes are eliminated. This group was an advanced class made up mostly of graduate students. The absence of poor records must therefore be attributed to two factors, namely, adaptation and skill in observing as a result of laboratory training and the more favorable conditions due to the smallness of the group. This points to the conclusion that in a large class of inexperienced observers most of the records poorer than .08 second may be due to lack of adaptation to the test or to disturbances in the room. The fine records on .02 second in a few cases of a large group may be due to the fact that in the larger group there are more chances of finding cases of exceptional ability.

To determine whether or not the distribution would be altered under even more rigorous and favorable conditions the same 13 observers were taken individually into the dark-and-quiet room where the test was made with most rigorous experimental control of the environment. The result is shown in Fig. 5b. which takes the same general form as 5a. The average is practically the same, .055 for the group and .054 for the individual tests. The Pearson coefficient of correlation between their individual records and those made in the group is .85, P. E. r .058, which would seem to indicate that both measurements are reliable.

To determine further the validity of the large group measurements 50 from the 200 in the first large group were taken for very careful individual measurements in the light and sound-proof room. The distribution for these is shown in Fig. 5c. The average threshold for these same observers in the group test was .07 second whereas their average in the individual tests was .05 second. This proves that the individual test is more reliable than the large group test because it gives finer records. The coefficient correlation between the two sets of measurements is however low, being only .23, P. E. r .07; that means that not all cases show improvement in the individual test and that there is relative instability in the records. That this instability is greater for the poorer records is proved by the fact that the average record for the best 20 in the above group of 50 was .054 second in the group and .047 second in the individual tests—a very small difference—and the correlation of the group and the individual records for these 20 best cases is .50 P. E. r .10 or more than twice as great as for the whole group of 50 cases. The significant thing in this is that it proves that ordinarily we may trust the finer records, whether in group or individual tests; the uncertainty is largely in the apparently poor records. This view is further supported by the distribution in that the better half of the surfaces of frequency is relatively stable for all the cases here considered.

It is still further supported by the results obtained from taking individual measurements of the 40 whose records were the very poorest in the last large group measurement. These results are shown in Fig. 5 d. The average threshold being .055, whereas in the group test, for the same 40 observers, it is .106. The correlation between the two sets of measurements is only .24, P. E. r, .085.

It is therefore probable that the poor records in the large group measurement are due to some cause other than a lack of appreciation of time on the part of the individual, and that this cause lies in the fact that with the large group there are within the group and peculiar to it sources of error that are not present in a small group or with the individual alone.

To determine the constancy of the record of an individual and the effect of practice, eight observers were given eight successive daily individual tests. After the first day in this series, only the two steps which were closest to the observers threshold of the day previous were used and in each of these 50 trials were made. The results are shown in Table I.

TABLE I. The effect of practice

Obs.	1	2	3	4	5	6	7	8	Ave.	% Gain
1	.051	.039	.039	.027	.030	.023	.024	.028	.032	45
2	.060	.063	.055	.032	.032	.032	.030	.030	.040	50
3	.052	.046	.042	.040	.040	.035	.037	.036	.041	29
4	.045	.047	.039	.032	.032	.028	.028	.025	.031	45
5	.039	.038	.039	.030	.040	.038	.039	.035	.037	11
6	.073	.064	.057	.050	.044	.043	.045	.044	.052	40
7	.040	.037	.040	.040	.034	.034	.039	.030	.037	25
8	.040	.038	.038	.037	.034	.034	.030	.029	.034	27
Ave.	.050	.047	.044	.036	.036	.033	.034	.031	.038	34

These eight observers were of approximately average ability, as may be seen by comparing the records for the first day with the form of distribution in the norms established for groups as shown above. In every case there was improvement as a result of the practice. The amount of gain for each individual, the average gain for the group for each successive day, and the daily fluctuations in the individual records may be seen in the Table.

This unquestionable gain with practice proves that the test was not elemental. The introspections also confirm this view, showing that the real difficulty is in the point of view, the method of imagery, the strain of attention, the method of counting, or some other such feature not essential to the simple experience of time sense. It is factors of this sort that form the basis for improvement by practice. To the extent that they are present this measurement, as a psychophysic test, is vitiated. Our aim is to make the test elemental, so that it shall be adapted for use in the measurements of individual difference. To this end the records and the observations in this series of measurements show that we must eliminate group distur-

bances and simplify the required mental attitude and strain to such extent that a test approximating the physiological limit shall be attainable without practice. As has been demonstrated in the above experiments, the first requirement may be complied with by making the test by individuals or in small groups in favorable surroundings. The second requirement must be met chiefly by simplifying the attitude of observation and the method of replying or recording. To do this our next step will be to take an individual test in which the interval-making sounds are not broken up into sets of ten, as here, or any other small group, but continued for a period of about five minutes each. This will secure better adaptation and will do away with the need of counting. Then the simplest sort of signal such as a motion of the hand may be used to designate those intervals which are recognized as short. Such simplifying of procedure should make this test approximately elemental. Norms are now being worked out on that basis.

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SOME STANDARDIZING TESTS ON STERN'S TONE VARIATOR[1]

BY

REUL H. SYLVESTER

The tone variator is a tone producing instrument of the blown bottle type. It was designed especially for use in gradual changes in pitch, and is made in different sizes. The one here used is of medium size and range, the bottle being 8 cm. in diameter and 17 cm. in length, with a mouth 1 cm. in diameter, giving a pitch of 200 v.d. to 400 v.d. The bottom of the bottle is a piston which moves upward and downward under the control of a gear and parobolic curve mechanism and can be set for different pitches according to a graduated scale. The tone is produced by a stream of air directed across the mouth of the bottle through a tube set at such an angle as to produce a whistling sound. The instrument is fully described by Stern,[2] who calls attention to some possible limitations to its accuracy, but offers no measure of reliability.

The experiments here reported were tests of the reliability of the tone variator, the intention being to use it, if it proved reliable, in studies of pitch discrimination. The constant air pressure was suppled by Whipple tanks.[3] Their pressure was regulated by weights and by a screw clamp applied to the soft rubber tube leading from the tanks to the variator. A water manometer, attached to this tube, gave the pressure readings. Where used in this report, these readings have been translated into grams per square centimeter. For reasons which will be given later, settings of the variator were always made in such a way that the movement of the piston was downward to the desired point, and by means of a special attachment the setting shaft control was firmly clamped each time. The pitch of the tone emitted was read by means of the

[1] These measurements were made in 1909. A synopsis of a report of them is given in the Proceedings of the Iowa Academy of Science for 1910, p. 195.

[2] Der Tonvariator. *Zeitschr. Physiol. u. Psychol. d. Sinn.*, 1902, XXX; 422-432.

[3] Whipple, G. M. A compressed Air Device of Acoustic and General Laboratory Work. Amer. Jour. of Psych. 1902, XIV p. 107 ff.

tonoscope.[4] In order to eliminate the interference at the mouth of the bottle by the funnel of the manometric capsule on the tonoscope, the electrical form of transmission, the phonette,[5] was used. Clamped with its lower edge in contact with the shoulder of the variator body, the phonette receiver faced the mouth of the variator in a position at right angles to the direction of the stream of air.

This apparatus, though complicated, was easily managed by one operator. With the tonoscope running, batteries turned on and the variator set at the desired pitch, the Whipple tanks were started and the taking of records began. After twenty readings had been recorded, the variator was set at another pitch and the record taking continued. With the exception of the resetting of the variator and the shifting of the tank weights, the apparatus ran continuously during two hours of experimentation. The pitch was read in tenths of a vibration, with accuracy. Every effort was made to secure the most favorable conditions for accuracy. It is not probable that the variator would be operated with such care in ordinary use.

Preliminary tests indicated that besides the changes in pitch which can be made in the intended way by moving the piston, variations of more than 15 v.d. may be made by varying the pressure of the air stream, and 10 v.d. variations may be made by changing the position of the mouth-piece. The results of experiments planned for a study of these two features are given in Table I, which shows the pitch variations for four scale readings, three pressures, and three mouth-piece positions. The limits of pressure and mouth-piece gap were determined by preliminary tests in which it was found that the extremes used here are the maximum ones at which the variator gives a fairly steady tone. Each pitch record given in this table is the average of between 40 and 50 readings. To this rather small amount of data, statistical checks of variability cannot be closely applied, but the mean variations given in the table are of considerable value. Their irregularity is largely due to the varying behavior of air currents in different settings, pressure, and mouth-piece gap combinations, some combinations causing more fluctuations than others. No records are given for the 400 v.d. setting because with the mouth-piece in the "flush" position there is a hissing sound which partly obscures the real tone and makes

[4] Seashore, The Tonoscope. (In this Volume.)

[5] General Acousticon Company, New York.

its pitch fluctuate, and because when used at low air stream pressure and wide gap mouth-piece position no sound is produced.

TABLE I. Tonoscope readings for the various combinations of pressures, settings, and mouth-piece gaps.

		3.8 mm. gap		1.9 mm. gap		"flush"		Average	
Pressure	Setting	Pitch	m.v.	Pitch	m.v.	Pitch	m.v.	Pitch	m.v.
2 gm.	200	187.9	.1	192.2	.2	193.8	.5		
	250	239.6	.1	242.4	.3	244.3	.3		
	300	288.0	.3	290.1	.3	292.4	.7		
	350	334.4	.4	340.9	.4	341.1	.7		
Average		262	.2	266	.3	268	.5	265	.3
3 gm.	200	191.7	.1	197.2	.3	202.1	1.2		
	250	243.5	.1	248.1	.3	251.1	.5		
	300	293.4	.3	296.9	.2	299.2	.6		
	350	345.4	.3	349.1	.6	345.3	.5		
Average		268	.2	273	.4	274	.7	272	.4
4 gm.	200	192.3	.1	201.3	.5	209.0	1.3		
	250	244.2	.1	250.9	.3	255.9	.4		
	300	294.5	.1	298.3	.4	302.4	.6		
	350	345.3	.2	352.0	.1	350.5	.8		
Average		269	.1	276	.3	279	.8	275	.5
Grand average		266	.2	272	.4	274	.7		

Inspection of this table indicates that the nine combinations of different pressures and mouth-piece positions, the 4 gm. pressure and a 1.9 mm. mouth-piece gap gives tones with pitches that are nearest the setting scale readings. The pitches, 201.3, 250.9, 298.3, and 352.0 vary from their settings of 200, 250, 300, and 350 an average of less than 1.5 v.d., while the averages of the other eight combinations vary from 2.2 v.d. to 10.1 v.d. from their settings. Therefore the intention of the designer of this instrument must have been that it be used with somewhere near 4 gm. pressure and a 1.9 mm. mouth-piece gap. If however, one disregards closeness to the setting scale pitch, he finds a lower pressure and a wider mouth-piece gap to be more desirable. With this 4 gm. pressure and 1.9 mm. gap the tone sounds forced and there is a prominent hissing in it, especially at the higher pitch settings. The tonoscope shows that under these conditions the pitch continually fluctuates. Obviously this is responsible for some of the larger m.v.'s in the table, it being impossible to catch the readings on the same phase of the fluctuations. When the mouth-piece is in the widest gap position, the pitch fluctuates least, the m.v.'s are least and to the ear the tones are the most clear, smooth and pure. Therefore this wide gap is more reliable than the 1.9 mm. gap. Evidence in favor of the lower pres-

sures is not found in the m.v.'s but to the ear the tones are clearer and purer.

Careful study of the table reveals no important tendencies that are obscured by condensing the data into averages. Hence the use of averages in the following consideration. They show first that an increase of pressure causes a rise in pitch. This is more marked in the lighter pressures, an average rise of 7 v.d. (265 v.d. to 272 v.d.) resulting from changing the pressure from 2 gm. to 3 gm. while, from changing the pressure from 3 gm. to 4 gm. the rise in pitch is but 3 v.d. (272 v.d. to 275 v.d). Averages in the table also show that when the mouth-piece is at the widest gap position at which a sound can be produced, the tone is comparatively low and changing the mouth-piece toward the "flush" position raises the pitch. This is more marked in the wider gap positions, an average rise of 8 v.d. (266 v.d. to 272 v.d.) resulting from changing it from a 3.8 mm. gap to a 1.9 mm. gap, while for changing it from a 1.9 mm. gap to "flush", the rise in pitch is but 2 v.d. (272 v.d. to 274 v.d.).

Mention has been made of the varying behavior of air currents at different settings. For this size bottle there seems to be the least disturbance at the 250 v.d. setting. Again using averages (and these averages as well do not smooth out or bury any important tendencies) one finds that for the 250 v.d. setting, the average of all records is 247 v.d. with a m.v. of .3 v.d. and that for the settings 200, 300, and 350, the averages of all records are 196 (m.v. .5), 295 (m.v. .4) and 345 (m.v. .5) respectively. In other words the pitch varies less from the scale reading than do the pitches produced at the other settings, and the mean variation is less. Hence the conclusion that a variator of this size is most reliable with the length of air column which gives a pitch near 250 v.d.

So much for conclusions from quantitative results. Certain general observations should be added. The manner in which the instrument's mouth-piece is attached is very unsatisfactory. It should be absolutely firm, accurately adjustable, and provided with a setting scale showing the width of the gap, or in some other way indicating the position. The piston sometimes settles downward. For the above experiments it was necessary to fit it with a clamping device. The securing of a steady stream of air is a serious problem. As shown in the tables, a slight pressure change causes a consider-

able change in pitch. The Whipple tanks are perhaps the best contrivance available, but they demand close care and are at best an occasional hindrance to the experimenting.

From the results of these tests, it is obvious that the tone variator can be relied upon only as an instrument of approximate pitch and relative intervals, and that it is not suitable for use in research which requires accuracy in pitch. The fact that its pitch varies with pressure and mouth-piece position, and probably with temperature, humidity, and other conditions makes an absolute reading impossible. A variation in one of these conditions would throw any scale out of proportion. But even if it cannot be relied upon for careful quantitative work, it is a desirable piece of apparatus for the psychological laboratory. Its loud clear tone and its ready manner of changing pitch make it especially valuable for general class experiments and demonstrations of consonance, beats and combination tones.

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STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO

An Experimental and Introspective Study of the Human Learning Process in the Maze

BY

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CONTENTS

I. INTRODUCTION

The incentive to the experimentation reported in this paper was an interest in the problem of correlating human and animal learning behavior. The most obvious and direct method of approaching such a correlation is by way of an objective test that will elicit similar reactions from the animal and the human being. The maze was suggested to me as the most convenient laboratory device for that purpose. Whatever may be the merits of the maze test, it at least affords a basis for a comparison of the activities of an inclusive range of animal types.

The dearth of published accounts dealing with the normal human adult reactions in the maze led me to believe that the investigations should be initiated in that practically untried field. Such a series of tests promised, in the first place, a set of learning curves that would invite speculative comparison with the animal maze curves obtained by Watson, Carr, and others. In the second place, the project suggested the possibility of interesting introspective results. The question of whether or no the subjective phases could be utilized for immediate purposes of correlation was thought to be entirely beside the point. I assumed that the objective results of a learning process which involved conscious functions could be explained adequately only in terms of those functions. Accordingly, the introspective reports have received the larger share of the emphasis, in conducting the experiments as well as in formulating the results.

The present investigation purports to be nothing more than a preliminary study of the general problem. It was begun in the hope that it would include an attempt at the correlation mentioned; it was finished after having submitted to laboratory test only the more obvious questions suggested by the title.

I have omitted the customary bibliography from this paper. The list of references bearing directly upon the subject is exceedingly meagre, and an attempt to give a complete bibliography of the learning process would scarcely be warranted in an account

of a very limited investigation of one phase of the topic. An excellent list is given by Ellison [Ped. Sem., 1909], and a more extensive one is furnished by the Clark University, "Bibliographies of Experimental Pedagogy".

II. EXPERIMENTAL SECTION

The experiments which furnished the material for this discussion were begun in the fall of 1909 and were continued through three academic years. All of the work was done in the psychological laboratory at the University of Chicago, with the exception of Experiment II. The mazes employed in the laboratory were designed by the experimenter, and were constructed by the technician of the department. Two types of maze were used—the pencil maze, and one through which the subject walked. A number of different pencil mazes were employed, which are described in the respective accounts of the different experiments.

The subjects were all adults, either graduate students or members of the faculty in the departments of psychology and philosophy. Throughout all the experiments they were blindfolded while learning the different mazes. The learning was by trials, and after each trial were recorded, (1), time, taken by the stop-watch, (2), errors, (3), description of behavior, (4), detailed introspection. The subject was each time asked to give as complete an introspection as possible, and was then quizzed by the experimenter.

The following served as subjects: Professor J. R. Angell, Dr. J. W. Hayes, Dr. Mary H. S. Hayes, Dr. Grace M. Fernald, Dr. Mabel R. Fernald, Dr. Ethel M. Chamberlain Porter, Miss Sarah M. Ritter, Dr. H. F. Adams, Dr. W. S. Hunter, Mr. R. B. Owen, Mr. E. W. Burgess, Miss Carrie Nicholson.

The investigations comprised a series of four principal experiments, which are described below under the headings of Experiments I, II, III, IV. In connection with the first a series of supplementary tests was conducted which are referred to as Tests 1, 2, 3, etc., and which are described at the close of our account of Experiment I.

A. EXPERIMENT I

The maze used in this experiment is referred to as the "Normal" maze, for two reasons: (1), it is as exact a duplicate of the modified Hampton Court maze used by Watson and Carr with the white rat as practical convenience permitted; (2), it was the pencil maze employed in our endeavor to ascertain the general course of the learning process. All of the other mazes used, with the exception of the one described in Experiment II, were designed with reference to a study of special aspects of the learning process.

1. Apparatus and Method

(*a*) *Description of maze:* The maze consisted of a sequence of paths and cul-de-sacs in the form of grooves cut through a board 5/16 inch thick. The grooves were 1/2 inch in width, and the board was 16 by 24 inches in size. A diagram of the maze pattern, in correct proportions, is given [Fig. 1.] on the opposite page, together with the numbers and letters used in our description to designate the various paths.

The maze rested upon a base of plate glass, and between the two a sheet of paper was placed, so that as the subject traced through the path with a pencil, a permanent graph was preserved on the paper of all his movements in the maze. The glass in turn rested upon a heavy table, upon which the whole apparatus was securely clamped, in a fixed position, marked out on the table. The subject sat in a straight-backed, comfortable chair during the trials, at a distance from the maze best suited to his ease and convenience. The starting box of the maze was directly in front of him, at a position approximately even with the mid-line of his body; the side BC of the maze was to his right. The position of the chair with reference to the maze and table remained constant throughout the experiment, and the position of the whole apparatus in the experimental room remained the same.

(*b*) *Method:* Each subject made one trial a day, at a fixed time, for six days in the week. During the progress of the ex-

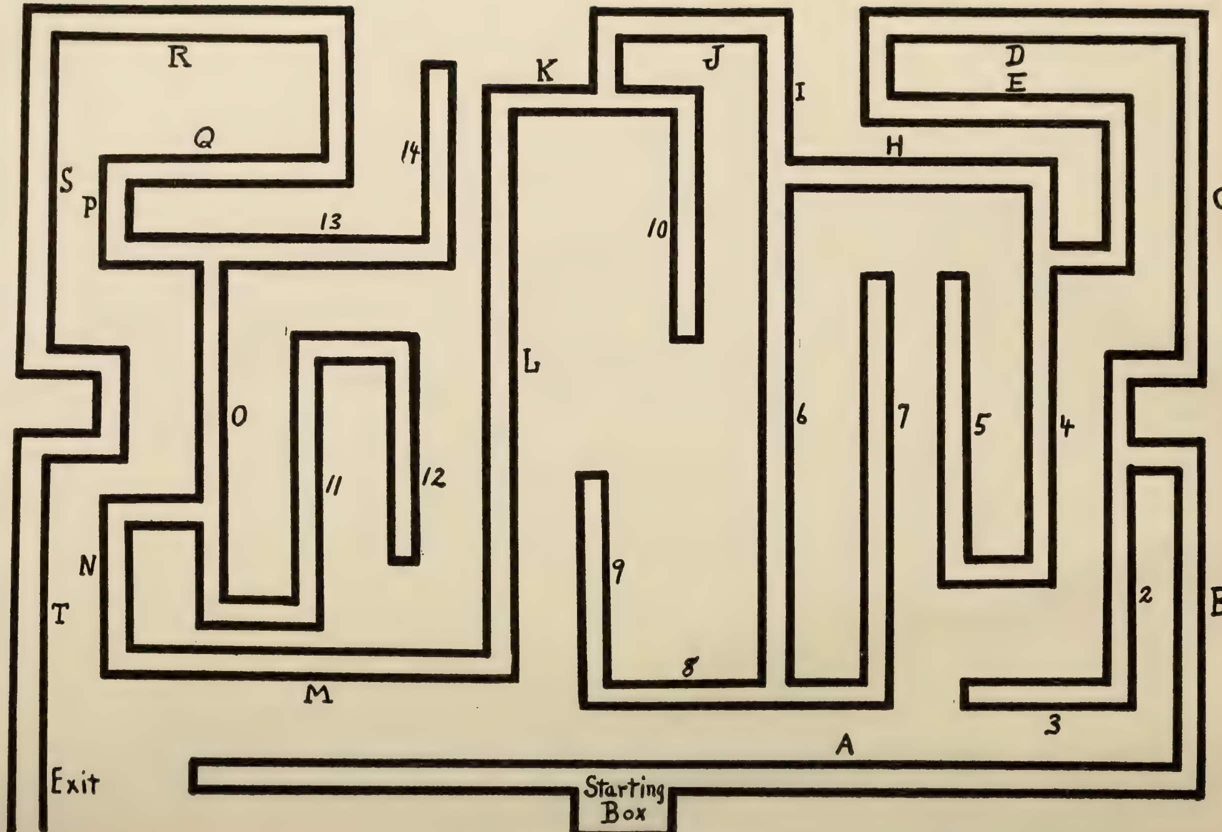

FIGURE 1. Normal Maze.

perimentation, various exigencies made a rigid adherence to the time schedule impossible. When these deviations from the routine procedure were responsible for discernible modifications in the learning process, the fact is noted in the discussion. With the exception of one subject only, not more than one trial per day was permitted each subject.

For each trial, the subject came into the experimenter's room, where a screen prevented him from seeing the apparatus. He was seated, and blindfolded with a pad of black China silk stuffed with cotton, that effectually excluded all light. He was then led around the screen, seated at the table in front of the maze, and given a pencil, the point of which was placed in the starting box by the experimenter.

The instructions and directions given each subject at the beginning of the first trial, to be observed for each trial, were as follows: (1) Hold the pencil in a position as nearly vertical as comfort will permit, and at the spoken signal, "start", endeavor to find the exit by tracing, keeping the pencil-point in contact with the floor of the maze (i.e., the paper) until the trial is completed. (2) Hold the pencil in any natural way desirable, but do not let the hand or arm come in contact with the maze. (3) Absolute freedom is allowed as regards retracing, stopping in the maze in order to think, or to relieve fatigue. (4) Time is to be counted from the starting signal until the exit is reached. "Time out" will not be granted except for unforeseen exigencies. All errors are to be recorded. An error is defined as a *false turn:* e.g., turning into a cul-de-sac from the true path, making any turns inside the cul-de-sac except those involved in retracing towards the true path, making turns on the true path in a direction away from the exit. (The subject of course learned to discriminate the true path from the false only as he made progress in the learning.) (5) Employ any learning technique available or desirable: that is, freedom is permitted each subject as regards thought processes or distribution of attention. Speaking aloud during the trial is permitted, but the experimenter will make no comment during the trial. In general, each subject is to employ any technique consistent with the directions

given. (6) Subjects are not to think of the maze, or of learning methods, or to communicate with each other on the subject between trials. (7) The maze will be considered learned when the subject is able to trace the shortest route, provided there is more than one, without error, for five trials in succession.

2. Results obtained from Experiment I—analysis of the learning process

(*a*) *General account of the objective behavior of the subjects in the maze, and of the nature of the learning process:* It was obvious from the introspective reports of the first trial that the learning process was to be largely a conscious one. Each subject, without raising the question of the possibility of any other method, set about to develop an ideational control over the maze. Motor habits played an increasingly important rôle during the succeeding trials; and various subjects reported during the course of the experiment, that the pattern of certain segments, which they had learned to run without error, had never been completely apprehended. But with these exceptions, the ability to run the maze, with a decreasing number of errors, developed with the ability to image the path, and to describe it verbally or graphically.

As the blindfolded subject was engaged in the active task of exploring the maze with the pencil, he was the recipient of a definite sequence of kinaesthetic sensations, due to arm movement and strain, and cutaneous sensations, as the pencil tip was pushed along the side of the pathway, or was moved without contact with the sides, or was suddenly brought into contact with the end of a passage and stopped. This immediate sensory experience, however, was reacted upon in a perceptual way. The subject's attention was, not on kinaesthetic and cutaneous sensations as such, but upon pathways running in various directions, and the turns and branches of the pathways.

The subject's reaction was not merely a perceptual one. The experience of paths and turns was immediately translated into image forms, of a nature and to an extent varying with the individual. Verbal processes were elicited, by way of description

and comment. The subject set himself more or less actively to the task of discriminating true paths from the false, of retaining, out of the confusion of experiences, the memory of the true path; of organizing his knowledge gained from each trial in such a way that it could be adequately applied to the succeeding trial, for the purpose of effective control over the task he had undertaken.

(*b*) *Summarized characterization of behavior:* The following brief description of the objective behavior throughout the experment applies in general to each subject. The first two or three trials consisted in a seemingly aimless trying-out of paths that offered themselves, a performance that strongly suggested the efforts of a newly-caught animal to escape from a pen. After this period, the procedure was as follows: (a), a tendency to work over into the general course of the true path. In doing so, each subject entered various cul-de-sacs, to a decreasing extent as he made progress. He entered the cul-de-sacs directly by turning off the true path, evidently not being aware of the mistake until he reached the blind end. The activity that followed in getting out of the cul-de-sac was very similar to that observed in the first few trials. It did not cease by any means when the true path was reached. (b) There was a very general tendency for the subject to enter the same cul-de-sac in this manner for several trials in succession, to drop it for a number of ensuing trials, and then to fall into it again for a time. (c) Most of the subjects, especially in the first half-dozen trials, would go repeatedly to the end of such a long passage, as S for instance, mistake it for the blind ending of a cul-de-sac, and retrace. (d) The subjects early learned to stick to one side of the path in certain regions, for the purpose of avoiding dangerous places on the other side, or because they had learned that the first turn on the side followed was the correct turn in the path. (e) A general tendency was observed to increase the speed of the pencil movement, (1), in familiar and safe segments of the path; (2), in trial and error attempts to escape from cul-de-sacs, or at any time when the subject became hopelessly confused—not necessarily in cul-de-sacs.

The chief observable difference in the behavior of the subjects was in the matter of speed. E. C. P. and H. F. A. made the quickest movements; J. J. T. and G. M. F. moved the pencil very slowly. The other subjects in this respect ranged between these two extremes, each represented by two subjects. Each subject established the pace in the first trial that was consistently to characterize his behavior throughout the trials.

(c) *Summarized consensus of introspections:* A brief general characterization of the subjective aspects of the learning process, based on unanimous testimony given by the subjects, will serve to introduce the more detailed individual introspective reports. They all state that the maze learning process took the form of discriminating and remembering a definite sequence of turns and paths from the confusion of experiences that characterized the first trials. Different turns and paths as they become learned acquired familiar cues, due to: (a), the feeling of arm position, as being extended towards the left part of the maze, the upper part, etc.; (b), kinaesthetic feelings of the length and direction of arm movement in traversing any one path; (c), a projected tactual feeling at the end of the pencil, as it turned corners and followed the paths. This element was slightly emphasized except in the case of J. W. H.; (d), the image schema of the maze, as checked up by these various sense factors.

The difficulty of organizing an adequate control knowledge of the true path may be ascribed to two causes: (a), the great initial difficulty, that decreased as the subject developed his control, was a matter of memory. At any given time during a trial, the subject could remember fairly well the last consecutive 2-5 turns and paths. But it was very hard to keep in mind the memory of the turns traversed earlier. (b) The second great difficulty was that of discrimination. The subject would proceed on what he was more or less sure was the true path, and would suddenly find himself at the blind ending of a cul-de-sac. He had no way of telling which one of the half dozen immediately preceding turns had led him off the true path, even if he were able to remember them.

After the first half dozen trials, the difficulties became located at two or three definite regions in the maze. The subjects had learned that certain segments were relatively free from dangerous situations, (e.g., A-F and P-T). The running of these segments became habitual, in the sense that while traversing them, the subject's attention was anticipatory, concerned with difficulties ahead. Motor habit, and the mechanical construction of the maze itself, were in this manner important factors in the learning process. The habit element assumed an increasingly important rôle as the trials were continued.

(*d*) *Summarized reports of the individual subjects as regards the nature of the learning process:* In the following reports, the data bearing upon the learning method have been abstracted from the complete introspections.

(1) Subject H. F. A. At the completion of the first trial, he had a scheme of the path in mind that he represented by the drawing [Fig. 2]. He adopted in this trial, and followed consistently throughout the experiment, a working method which he describes as that of "conscious trial and error". Each day he attempted to work over in the general direction he knew the path to extend, with the aid of the memory of such specific segments as he could retain. He would follow any given path to the end, the "bump". There he might or might not get a cue for the proper turn, a kinaesthetic memory experience. If he got it, and it turned out to be the correct one, well and good; if not, he would work around in haphazard fashion, and eventually recover his bearings. He made no special attempt to reason out situations, or to formulate plans or theories. He did consciously attempt to discriminate and remember. He thinks he learned the maze, "as a rat learns it—assuming that it is conscious—by a trial and error method, and an associative memory control." Furthermore, he employed the same cues that a rat presumably utilizes—kinaesthetic and tactual experiences.

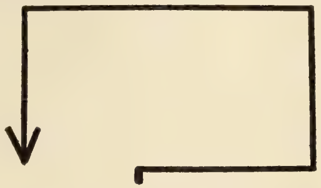

FIGURE 2. Drawing of Normal Maze, end of first trial, H.F.A.

(2) Subject M. R. F. She purposely adopted on the start a trial and error method, but with this, in the first trial, and throughout the ensuing trials, she assumed a decidedly rational, thinking attitude towards her task. She conceived the idea, (I),[1] of exploring one side at a time, but it did not prove to be a sucecssful procedure. The results of trials (I-IV) were general orientation—the general spacial relations were learned first. Like H. F. A. (and other subjects) she was alertly on the lookout for familiar passages, but she made more elaborate anticipatory judgments and plans—"The next time I reach this corner, I am going to turn north and see what happens." At (IV) she started out with the general working idea to eliminate useless movements. She planned, for instance, to avoid a "bad place" on the right by trying to find a new path around it. She thinks about the maze as she learns, in verbal terms, sometimes spoken aloud, such as: "Never went up here so far before; yes, this is O. K.; guess I'll try this path to-day." By trial (XVI) she believed there were four different paths in the maze; just how much is common route, she doesn't know. She thinks possibly the maze is being altered as she learns. (The experimenter was extremely careful in the preliminary directions to state that there might or might not be more than one true path. He made no mention of possible alterations during the learning.) The whole process was largely a series of discoveries and guesses. She used conscious exploration to check up her theories, but exploration almost inevitably resulted in getting hopelessly lost. In (XXII) she found herself in a familiar cul-de-sac, 6-9, and recognized it, but was sure she escaped from it by a new path. By (XXVIII) she is firmly convinced that there is more than one path—stated that while she used to come down a long path in the middle of the maze, (L), for the last three or four trials she has been entering it from the right, at a place much lower down than usual—that is, the path is perceptibly shorter than it formerly was. Therefore, she concluded, there are two ways of getting into it, and therefore two paths in the right part of the maze. She hit upon the idea of learning one of the paths in

[1] The Roman numerals in parentheses refer to the number of the trials.

the maze, and letting the others go. At (XXXIII) she has the situation under control—that is, she can go through one of her paths without error. At the conclusion of the experiment, she had not discovered the the nature of the other paths; but technically the maze was learned.

The reports of this subject present a distinct contrast to those of H. F. A. Her attitude was consistently a more rational one. She did more thinking about the problem. But she reports that practically her learning was by the method of trial and error, that the discoveries she made which were of vital importance were more or less accidently hit upon.

(3) Subject E. C. P. This subject was our most consistent and exclusive visualizer, and her overt aim from the start was to build up a visual image of the maze path for the purpose of control. To the extent that she accomplished this, she made progress in the learning. In (I) she became conscious that she was in the right side of the maze, and started to work over to the left. Her whole subjective process went on in terms of building up and reconstructing this visual pattern. The general shape was developed first, i.e., (IV) "The path is of the form of the capital M." Her attitude was consistently one of active attention, discrimination, and memorizing. At times she attempted to follow tentatively a few working ideas—like M. R. F., she attempted to "dodge" the cul-de-sac region on the right by trying to work around it. But she does not report much systematic thinking or planning. Her procedure was to go ahead, learn what she could by experience, and construct out of it as best she was able her control image. She aimed to make her actual movements as rapid as possible, to go cautiously in certain regions, but to depend upon excess motor activity as the best method of procedure. By (XXIII) she had a schematic visual image of the path, accurate as far as the correct sequence of turns is concerned. The errors she made after this trial were due, she thinks, to careless attention.

This subject's account offers points of similarity to the reports of both H. F. A. and M. R. F., but it seems to represent a third distinct type of method. Without the excess play of ideas of

M. R. F., she had that subject's attitude of active, concentrated attention and mental effort. In this respect she was a pronounced contrast to H. F. A., who from a comparison of the respective reports, seemed mentally more careless. Like him, however, she adopted a method characterized by its emphasis upon excess random activity, as the result of which the correct paths could be selected and retained.

(4) Subject M. H. S. H. The general working method of this subject is rather difficult to characterize. A rather extensive rational procedure that she indulged in, in connection with learning to avoid a certain cul-de-sac, is described in an ensuing section of the account of this experiment. Her testimony is to the effect that she proceded as a rule cautiously, with her attention alert for kinaesthetic cues. Her aim was to build up a control, in terms of a sequence of anticipatory motor images. This called for a rather active process of attending closely to kinaesthetic experiences, selecting out of them the ones concerned with sections of the true path, and organizing these into an adequate idea of the route. She indulged freely in verbal expressions, spoken aloud during the trial, but they were predominately by way of comment and exclamation, rather than description or systematic thinking.

(5) Subject J. W. H. The reports of J. W. H. as to method are very similar to those of E. C. P. They differ radically in one respect. This subject tended to rely upon motor habits as soon as they appeared, and let consciousness concern itself with other parts of the maze, or with extraneous matters, while E. C. P. put less emphasis upon the habit element than any other subject. She seemed to attend consistently to her visual scheme of the maze while traversing it.

(6) Subject G. M. F. This subject differed from all the others in developing an auditory-verbal formula as a control of the maze. In the actual constructing of this set of verbal directions however, she used the type of kinaesthetic processes and imagery described by M. H. S. H., and employed them in much the same way. That is, she did her thinking in these terms, rather than in verbal terms. She was still more conservative than the

subject just referred to in the matter of random exploration. Subject G. M. F. made very slow movements in the maze. She engaged quite consciously in what she called "wondering" or guessing, but made no great effort to reason out situations. She does not report attempts to try out different schemes, such as sticking to one side, altering speed, etc. In one or two instances, she undertook a systematic exploration of certain segments of the maze, but did not place much emphasis upon the value of such a procedure. She gave each trial an attitude of close attention; but her mental activity seemed for the most part to be concerned with discriminating kinaesthetic experiences, translating them into her verbal formula, and memorizing that formula.

(7) Subject J. J. T. The introspections of this subject indicate more fluctuations and irregularities than those of any other learner. For the most part she put her active attention upon her task, but in several reports "relaxed attention" is the predominant theme. Like the other subjects she was on the lookout for familiar cues, and in seeking them she put the emphasis upon the "long sweeps." She made no special effort at thinking out situations. She did at times attempt rather systematic exploration. But several times she indulged in what she called "willful and malicious" exploration: that is, with no special motive in view except curiosity. All of her explorations she reported futile. She did some theorizing, or rather, guessing—e.g., she conceived at various times that there was more than one path in the maze. But like G. M. F., she did not develop her guesses into theories, and systematically attempt to substantiate them.

(*e*) *General comments on the different reports as to method:* It is evident that the various subjects took different attitudes towards their problem. By attitude we mean simply the learning method the subject attempted to apply. Two opposed attitudes can be fairly well defined, in the light of the data given. One represents a more active, volitional attempt at thinking than does the other. The data also suggest another possible basis for a distinction of attitudes. Eliminating the matter of the amount of thinking, some of the subjects seemed to concentrate their attention upon the problem more than did others. On the basis of

the first distinction we contrasted H. F. A. and M. R. F.; on the grounds of the second, E. C. P. and J. J. T. It is to be emphasized that these distinctions are decidedly relative ones, but the introspective records seem to justify a tentative classification on these bases.

Assuming that there are differences in attitude, in the way the term is defined above, the question is strongly suggested, do these attitudes represent actual psychological differences in method? Each subject effected a process of mental organization of experiences that functioned in his successful learning of the maze. Was the nature of this organization and application of experiences essentially the same for all the subjects? It is significant that all of them reported that they had to resort to a trial and error method sooner or later; but it is quite possible that they underestimated the importance of the rationalizing they indulged in. It is conceivable that the adult human mind functions in such a way that no other type of mental behavior is possible for this special learning process. But it is likewise possible that the mind is of such complexity that it may approach this type of problem in a variety of ways. The question was thought important enough to be made the object of a special investigation, which is described in Experiment III.

3. The functioning of some of the specific activities involved in the learning process

(*a*) *Imagery, and sensory processes:* No special reference was made, in the account of method, to the various types of imagery employed by the various subjects. This was made, however, a special topic of study, the motive being to determine if possible the relative efficacy of the different types employed, provided there were indications that any one type or combination showed itself to be of superior value in the learning activity. It is possible that in the instances where any one subject employed a complex of different kinds of imagery, some of them were of functional importance, and some of them merely accessory. The question of how the imagery was used, was deemed to be of primary importance.

In our attempt to determine the imagery employed, we availed ourselves of the evidence presented by three lines of data: (a) detailed introspection on this subject was called for throughout the experiment; (b) an attempt was made to check this up by some objective tests, after the experiment was concluded; (c) one of the subjects was at the time of the investigation engaged in research on the problem of imagery diagnosis, in which she employed many of the introspectors who learned the mazes. Our records were checked up with the analysis she had made at the time.[2]

We had represented among our subjects a rather inclusive variety of image processes, complicated in all instances with sensory activities. The experience of tracing the path through a pencil maze necessarily elicits kinaesthetic and tactual factors. It was not to our purpose in every instance to distinguish sharply between the two factors, but we attempted to do so when there appeared any functional reason for the analysis.

(1) Characterization of the subjects with reference to imaginal and sensory elements employed: (i) J. W. H. Quite sensitive to tactual processes in the maze. Utilized a strong sense of egocentric projection—the tactual feeling projected to the end of the pencil. Less conscious of kinaesthetic factors. On the basis of these experiences he developed visual imagery for certain segments of the maze, and employed the tactual-visual complex as his conscious control.

(ii) M. H. S. H. A very pronounced and almost exclusive consciousness of kinaesthetic processes. While actually engaged in going through the maze her active attention was upon, (a), the motor sensations from the hand and arm, and to a less extent, from the body; (b), the unambiguous anticipatory imagery of the turn-to-come. We were not interested to determine whether this was purely imaginal or in part sensory in its make-up. It was a definite kinaesthetic anticipation of the segments of the path immediately in front of her, including one or at most two turns, of a more inclusive segment of the maze. Her idea of the

[2] M. R. Fernald: The Diagnosis of Mental Imagery, Psychol. Rev., Monog., Suppl., 1912, vol. XIV, No. 1.

maze was built up in terms of this kinaesthetic experience. Her learning, in its subjective aspect, involved the process of discriminating the sensory experiences concerned with the true path, retaining them, and applying them in the form of anticipatory images in succeeding trials.

(iii) G. M. F. was like M. H. S. H. in having strong motor imagery, and in being primarily conscious of kinaesthetic sensory experiences while running the maze. She was peculiar among all the subjects in that she built up, as she learned, a specific auditory-verbal formula, which, as checked up by the motor imagery, served for a guide. She testified that she could not dispense with this formula, even during the last trials.

(iv) M. R. F. employed a mixture of "vague, fleeting visual, scattered verbal, and indefinite motor imagery." At times one element would temporarily predominate. As a rule she was unable to determine the relative importance or extent of the different components in the complex.

(v) E. C. P. was a very definite and practically exclusive visualizer. She built up a clear-cut visual image of the maze, which she describes as schematic, in the sense that it was composed of "lines," rather than being an image of an actual wood maze, of any certain color, etc. Like G. M. F., she used her image as a guide for every trial, and ventured the opinion towards the close of the trials that she could never run through the maze without it.

(vi) J. J. T. found it difficult to introspect on her imagery. She was conscious of kinaesthetic complexes, but had no definite motor imagery, and little or no visual. She does not think she employs much imagery—what she knows about the mazes she simply "knows."

(vii) H. F. A. employed a kinaesthetic complex which he did not try to analyze into its imaginal and sensory components—i.e., his anticipations of turns were closely involved with the turns themselves. With this he used some verbal material. No visual or tactual.

(2) Some data on how the imagery functioned: The type or combination each individual employed was used throughout the

experiment, and was the same that the subjects reported in their statements made during the investigations of M. R. F., referred to above. There were no radical attempts to shift from one type of process to another. Each subject reported fluctuations in the amount used through the trials. In general, when the learner increased his effort to concentrate, or to study out situations, he reports an accentuation of image activities. To the extent that the learner relied upon the habit factor, the imagery was lost.

The image activities were either retrospective or anticipatory. One aspect of the learning process was the retention of the segments just traversed; the other was the application of this remembered experience in a succeeding trial by way of an anticipation of the turn or turns to come by means of which the subject was successful in making them correctly.

There were some differences reported as to the amount of path actually anticipated. E. C. P. would develop her image of the whole route, or as much of it as she knew, before leaving the starting box, after the signal to begin had been given. The two or three turns immediately in front of her would be more vividly expressed, however, as she went through the maze, while the "trail" behind would be practically ignored. M. H. S. H. would get her image of the turn-to-come when half way or two thirds of the way down the preceding path. The same type of image employed by G. M. F. would usually include several anticipated turns; but it was used in connection with her verbal formula, and included only certain segments of the maze. She elaborated her formula as she had actual use for each step in it when going through the path. H. F. A. reported that his cues for the turns came as a rule after the "bump," when the end of the path was reached. The other subjects gave more varying testimony as to the extent of path that was anticipated.

It is rather difficult to gauge the importance of the motor element. One would assume after reading the reports that the kinaesthetic processes played an important rôle with all the subjects, that the cue that was focal with M. H. S. H. functioned to some extent with all. This would of course imply a persistent

relationship between the kinaesthetic processes and motor habit. The definite motor image cue which this subject got some distance ahead of the actual turn was probably the same that J. J. T. and H. F. A. acted on in their controls; although in their cases it was not acted on as such, and it was closely identified with the actual act of turning. The tendency for the whole kinaesthesis to become habitual probably accounts for the phenomena of habit reported almost universally in the latter part of the learning. The introspections seemed to indicate that the kinaesthetic process behaved differently with different subjects, and we have some reason to believe that it actually functioned to a different relative degree with different subjects. It is interesting to note that E. C. P. consistently reported that she did not consciously utilize kinaesthetic processes as a guide at all. She was unique among the subjects in this respect. It is also significant that she also reported little or no automatism or habit, again differing from the other subjects. E. C. P., it is to be remembered, was our most consistent visualizer.

In general, as far as our introspective data are valid, the different image processes, when represented in one individual, reinforced each other. It is interesting to note that while G. M. F. reported that her verbal formula was her immediate control, it is hardly sufficiently explicit to guide a subject, unfamiliar with the maze, through its paths. It reads, "Along in this alley—around corner—here—straight up, going to turn to left—now I go down—back to right—now here I must be careful and stick to upper side of alley," etc. This is for the region A-H. Evidently this set of directions alone could scarcely suffice for safe passage through the maze. The inference is that it was probably reinforced by motor cues, feelings of familiarity, etc.

In one or two cases we found instances of an image conflict. G. M. F. reports such a case where the formula told her to go one way, while a motor image directed her into an opposite path. The verbal cue turned out to be correct. M. H. S. H. in two instances reported a conflict of motor images. This was after she had just learned to avoid a cul-de-sac, and one tendency represented the habit effect.

(3) The question of the relative efficacy of the different types of imagery employed: It was impossible, after a comparison of the objective records and the introspections, to make any positive correlations between efficiency and the type of imagery used. In the first place, it is obvious that too many complications enter into the learning. Two of the best learners, E. C. P. and H. F. A., used very divergent image processes. Two of the subjects who employed similar image controls, H. F. A. and M. H. S. H., have extremely opposed objective records.

After the maze was once learned, any one type of control was as efficient as any other, as far as the criterion established for this experiment was concerned. Evidently, the relative superiority of any one type of thought process, if such existed, must have asserted itself during the earlier part of the learning.

There is some evidence to indicate that those with predominant motor imagery tended to establish stronger motor habits than the others. M. H. S. H., in the distraction tests, to which reference is made later, was exceedingly successful in going through the maze when her attention was mainly concerned with other things. E. P. C. failed in the same test. But this does not by any means imply that a strong tendency towards the establishing of non-conscious habits is beneficial in learning a maze. Habits worked for harm as well as for good. In the case of M. H. S. H., a cul-de-sac became incorporated into the true path and was habitually run for a number of trials before the mistake was discovered, each time being responsible for a number of errors.

It may be that visual imagery is intrinsically better adapted to express spatial relations than kinaesthetic or verbal. It is more comprehensive—a verbal image must unroll itself in time, as must also kinaesthetic one, while the visual image is presented more instantaneously. E. C. P. anticipated more of the path before reaching it than did any other subject; H. F. A. did less than any other. Both had good records.

The conclusion was strongly suggested to the experimenter in the light of all the data at hand, that the learning depended upon the ability of the subject to organize the experience pre-

sented by the objective act of going through the various paths in the maze, rather than upon any type of imagery employed to retain these experiences. Several indications were found to the effect that different subjects *used* the same kind of imagery in different ways—e.g., M. H. S. H. and H. F. A. with motor imagery; G. M. F. and M. R. F. used verbal material in different ways.

(*b*) *Habit and Attention:* Very early in the series, in some instances in the first half-dozen trials, subjects began to report that certain segments of the maze were being run with the attention directed upon regions ahead, or upon foreign matters. This type of thing was the more emphasized as the trials continued, until various subjects, after the last few trials, reported that most of the maze was traversed with focal attention wholly concerned with other things.

This habit tendency developed in connection with two different kinds of situation in the maze: (1) certain segements, such as A-F, which had presented relatively few opportunities for error, never at any time became the object for much attention. Such regions early were reported as "almost automatic"—"attention concerned with the cul-de-sac ahead." Another type of thing occurred, of the same nature as far as the subjective aspect of it is concerned, of which the reports of M. H. S. H. afforded an excellent example. This subject incorporated the cul-de-sac path 6-8-9 into her true path without realizing what she had done. Ordinarily, the end of a blind passage afforded a sufficiently characteristic cue to inform any subject that he was in a cul-de-sac. In her case, however, the passage to the end and back was sensed merely as a turn in the true path, and as a result, this part of her path became habitual before she discovered her mistake. (2) In other instances, segments which were learned at considerable expense of attention and study eventually became habitual. The path F-K was the common instance of this.

The behavior of attention has already been referred to in our report of the image processes. It either, (1), kept pace with the actual activity in going through the maze, (2),

"lagged behind," or was retrospective, engaged in the attempt to retain the knowledge just acquired, or (3), was anticipatory. In a relative way, these three types of the play of attention made their appearance in the order named. Ordinarily, the progress of any trial, or the traversing of any one path, would elicit a back and forth play of attention between the region ahead and the "trail" behind, checked up by constant reference to the path the subject was actually engaged in making. This behavior is characteristic of all the subjects.

A rather surprising divergence of behavior in the matter of habit was in evidence. E. C. P. constantly testified that her visual scheme was her control, and that she employed it in her last trials as consciously as she did in the first. She was quite sure that she would never be able to dispense with it. M. R. F. also emphasized to a very small extent the phenomenon of unconscious running of segments of the maze. But this subject was engaged in an active series of studies throughout the learning, while E. C. P. was one of the first to build up an adequate idea of the true path. G. M. F. reported her verbal formula essential to the last.

In general, the appearance and predominance of the phenomenon of unconscious running seemed to be correlated with the relative reliance upon kinaesthetic factors, but J. W. H., who next to E. C. P. employed visual imagery, emphasized the habitual, unconscious elements more than any other subject. J. W. H., also, it is to be remembered, reported on the introspective side a minimum of kinaesthetic processes. He thought they were decidedly subordinate to tactual ones. But M. H. S. H. who relied so exclusively upon motor imagery, while she emphasized in her reports habitual running, maintained at the same time that it was a conscious performance. The appearance of habit is hardly to be ascribed to the number of times the maze was run. Those who did report it, testified to its appearance early in the series. Neither is it obviously correlated with speed, as a comparison of the different behaviors show E. C. P. and J. W. H. similar in this respect. It is not to be explained in terms of the amount of attention put into the learning as a whole.

J. J. T. and J. W. H., who occupy rather extreme positions in this respect, both report strongly the rôle of habit, in the sense of unconscious running.

The data, by way of summary, indicate that various subjects varied in their reliance upon the habit factor. In the instances of its extreme predominance, the subject could safely depend upon it to carry him through difficult regions of the maze while he was attending almost exclusively to extraneous affairs. In other cases, although habit facilitated the task of going through the maze, some attention to the difficulties in the route was necessary. Finally, two of the subjects reported that an amount of conscious attention was demanded in each trial, which did not seemingly diminish as the result of repetition.

(*c*) *Discrimination and Association, Memory and Recognition:* The processes involved in selecting segments of the maze which belonged to the true path out of the confusion of experiences which in the first trials presented themselves, and arranging them in the proper sequence, obviously constituted the main organization that the subject was called on to accomplish.

It has already been stated that certain passages early acquired familiar "tangs," due to their direction, extent, position, etc. Practically every passage had something analogous to a local sign, due to the quality, strength, and combination of kinaesthetic and cutaneous experiences occasioned by the act of traversing it and attending to it. But the subject had not only to attend, but to remember. At some given time in the early stage of the process, he would find himself stopped by a blind ending, say the termination of path 9. Assuming that at that moment he had rather definitely in mind, in some image form, the memory of 9-8-6-H-G-F just traversed, he would not necessarily have any cue as to which of the turns between these passages had led him off the true path and into the cul-de-sac. This was the type of difficulty reported universally. Add to this situation the fact that in the earlier trials the subject had in mind only a confused memory blur of the immediate past experiences, and the nature of the conscious side of the learning is easy to conceive.

The learner had two things to do in such an exigency. He

must in the first place escape *from* the situation. In many instances, especially in the earlier part of the learning, he deliberately resorted to a random trial and error method, letting any attempt to get a mental hold on the situation go where it would. But if he were to build up an adequate knowledge of the path, he must in addition learn how in the future to *avoid* such a situation.

It was then the concern of the subject to retrace, attend closely to his cues, and recognize the regained path as soon as possible. The chances are that in doing so he would go into 7 and back. Retracing from there (possibly reëntering 9) he would finally find 6, turn either into H or continue I, and make a number of ensuing turns before he found his bearings. Possibly in the next trial, in an attempt to escape from this region, he would turn down 4. At the end of 5 he would find himself, of course, worse off than he was in the previous trial.

There were two methods in evidence by which the subject learned to avoid this cul-de-sac 6-9. One was the actual process of learning the whole situation by repeated exploration. The other was the result of turning directly from H to I by accident, without at the time recognizing it as the correct thing to do, remembering this variation in the route, and afterwards ignoring passage 6. In this case the cul-de-sac was not learned at all. 6 was simply the opening from clear sailing into an unknown region of danger. This process of falling into difficulties and learning how to avoid the place on the true path that was the location of the difficulty, called for the discriminating, associating, memorizing processes. But it was obvious that two subjects could learn to avoid the same region by two methods, one of which called for a more active play of these processes than the other. Unfortunately, the subject, although he was aware of the two methods of escaping difficulties, could not choose between them. The second type of thing was invariably accidental, or due to causes which his introspection did not comprehend.

Thus while specific acts of memory and discrimination entered into the learning process, they did not constitute it. The background of preconceptions, the effect of habit, the play of

ideational activity, all entered into the mental organizing involved in these relatively simple forms of learning. The experimenter devised a series of extended tests designed to guage the ability of the different subjects in the matter of sensory discrimination and memory, under conditions as similar to those involved in Experiment I as possible. The results of these tests offered no basis for an explanation of the efficiency in the maze. As was noted above, E. C. P. was able to describe the path before it was technically learned. J. J. T., when the experiment was completed, was not able to describe accurately the sequence of turns. The relative lengths of paths were grossly distorted, even in regions where the path had been carefully worked out, by all the subjects.

(*d*) *Illusions:* It is convenient to make reference under a separate heading to the fact that the number and nature of the misconceptions the subject formed of the maze were startling. Lengths of paths were over- or underestimated frequently by one half. Certain areas of the maze were violently distorted as to position and relative size. The fact has already been referred to that one subject believed in the existence of four different paths in the maze. This may not properly be called an illusion, but we found it impossible to establish boundary lines between errors in sensory discrimination, illusions, and misconceptions due to guesses or theories.

That these illusions often played a definite rôle, generally to the detriment of the subject's control over the learning, is evident from the reports. Sometimes an immediate increase in time and errors resulted, but often the objective records show nothing of the misconception. H. F. A. (VII) ran up 15 errors due to the fact that he first noticed in this trial that M was a relatively long path: he had always considered it a short path, or rather, had not had his attention called to it at all. At this trial he entered it, was surprised at its length, concluded he was astray, and retraced, in an attempt to find the path he formerly took in getting from L to the left side of the maze. M. R. F.'s conception of the four paths resulted in needless exploration, and her error and time curves were correspondingly enlarged. J. J. T.

(X) found the relative proportions of the passages in the right side of the maze distorted, but since her idea of the sequence was unaltered, the discovery made no appreciable difference in her records. G. M. F. (VI) decided that there were two routes from the entrance to the exit. In this case, the judgment was the result of a careful exploration of the region. After the judgment was formed, however, it did not influence her error records, for she simply followed up whichever path she "happened to be on" in this region, and since there was only one, her record is clear.

As for the cause of these illusions, the introspections seem to indicate the following: (1) errors in pure sensory discrimination; and, (2), the fact that the attention frequently became focal at times in passages usually run without definite notice. Hence the subject perceived things he had been blind to before, and assumed that he was in a new situation.

(*e*) *Affection and Emotion:* The process of learning the maze elicited at times affective reactions that in some cases obviously influenced the progress of the subject. There were sufficiently in evidence, as one would assume, the feeling of discouragement in times of difficulty, and the corresponding state of elation when the difficulty was overcome.

The extremely disagreeable, hopeless feeling of being baffled very often resulted in a definite change of activity or method. Repeatedly, the subjects would find themselves in cul-de-sacs, would try at some length to extricate themselves by a study and exploration of the region, and would then resort in disgust to a random trial and error procedure in an effort to escape at any cost. It was extremely difficult to gauge the practical effect of this method. Quite often it resulted in discoveries of permanent value.

In one or two cases excitement over personal affairs distracted the subject's attention and the learning process suffered accordingly. Thus E. C. P. had just passed the German examination required of candidates for the doctor's degree a half hour before trial VII, and was in a state of considerable elation. Her record that day includes 75 errors, the most made by any subject in any trial.

This affective aspect showed itself most usefully in the incentive for quick learning. The sense of rivalry among the subjects was acute, and the learning was almost literally regarded as a test of intelligence. The experimenter kept each of the subjects sufficiently informed of the progress of the other learners to maintain this motive at a working level.

(*f*) *Thinking and Reasoning:* The reports of all the subjects have implied in them something more than the specific activities referred to in the preceding sections. Various indications have already been given to the effect that the kind of mental organization elicited was of an exceedingly complex sort. In some cases this amounted to overt attempts at logical reasoning; in all instances it seemed to have involved in it the rudiments of the higher thought processes.

While the problem was one in which a minimum of direct experiential background could be drawn upon by the subject, he nevertheless did avail himself of the general intelligence of human experience, in a way that an animal could not possibly do.

His general attitude, in the first place, was that of a learner who knows he has a problem to solve—certainly a rather important factor when one considers that the active effort of the learner was the thing that primarily characterized his activities.

Numerous specific reports indicated that the subject was able to call to his aid knowledge of a more general sort, and that he was able to conceive of working schemes not elaborated as the result of actual experience in the maze. Thus E. C. P. (IV) said the path was of the shape of the capital M. M. R. F. on the start conceived the idea of exploring one side of the path first.

An example of the realization and solution of a difficulty in a way that disclosed the attempt to rationalize the problem, and the amount of trial and error actually employed in its solution, is found in the experience of M. H. S. H., which is different from a number of instances given only inasmuch as it was more extended, and was elaborated more consciously.

During the first twenty trials, almost without exception, this subject went daily into cul-de-sac 6-7-8-9. During this time she constantly reported a tendency for familiar parts of the route

to drop into the habit class. Until about (XXV) she would go into this cul-de-sac every day, and retrace out of it, under the impression that she had made no errors. By (XXV), however, some cue given at the end of 9 made her aware of what she had been doing. She immediately began to study the situation. Since she had incorporated this into her true path—so she reasoned—how was she going to avoid it, since she did not know how much of the territory was cul-de-sac region? And since the cul-de-sac runs "up" (she meant of course either 6 or 9) and the true path also runs in the same direction, how was she going to be able to tell which was which? Her conception of the situation, as she explained, was that she gets into the cul-de-sac by turning *up* from some point on the true path. From (XXVI) to (XXXI) the conditions of affairs did not change. She had no feeling of being in error until she reached the end of 9, and practically restated her reasoning process: "I have to turn up to get out of the thing, this cul-de-sac also turns up, so there is no way to tell which is cul-de-sac and which is true path."

Curiously enough, in (XXIX) she ran the maze without error, but reported the whole affair as almost unconscious, and she was therefore unable to explain how she escaped from the cul-de-sac.

At (XXXII) the scheme occurred to her to avoid the region by turning up sooner in the path: "Will try this next time." In (XXXIII) she tried it but found no path, (i.e., in H) leading in that direction. However, she said at the end of this trial that the plan seemed logically sound, so she would try it again. In (XXXIV) she attempted it once more, with the same result, and reported, "Tomorrow I will turn *down* sooner than usual." (That is, some path that would lead from H to L, turning down from H.) This also failed. In (XXXVI) she got half way down 6, when the idea occurred to her that she had gone past the customary path downwards, that she was now in a new region, and that she had, therefore, better retrace.

In her next trial, the true situation occurred to her: not to turn down at all, in the path G-J. This solution of the problem came as the result of the fact that she had unintentionally and

accidently turned up in the trial previously made, and avoided the cul-de-sac.

This report suggested to the experimenter the advisability of making the amount of reasoning possible in such a situation the object of a more extended study, and with that purpose in view, the third experiment was designed.

4. A comparison of the subjective aspects of the learning process with the objective records

The criterion of five consecutive trials without error as an indication that the maze was learned was an entirely arbitrary one, even if the subjective aspects were not considered at all. Had the test been four trials, G. M. F. would have had the maze learned at trial XVII instead of at XXIII. If the standard had been determined at six trials, E. C. P. would have needed at least six extra trials. Several subjects ran occasional perfect trials in the earlier stages of the learning, before they were able to approximate a detailed description of the maze—E. C. P., XI; G. M. F., XIV; J. J. T., XIV; H. F. A., VI; J. W. H., XII; M. R. F., XIV.

Had a psychological criterion been attempted to determine when the maze was learned, e.g., the standard of ability to describe the true path, the test would have been just as arbitrary. With any subjective standard, the time and error records would be different from those based upon the objective test adopted—the number of trials would have been increased for some, decreased for others. E. C. P. described the true path accurately at the close of trial XXIII. H. F. A., when the maze was technically learned, described F as extending up from E instead of down, turning to the right, and then turning down to meet H. J. J. T. drew the reproduced diagram [Fig. 3] to represent all she knew of the true path,

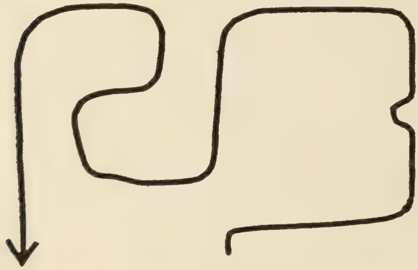

Figure 3. Drawing of Normal Maze, when learned, J. J. T.

after she had run it five times in succession without error. M. R. F. could describe the proper sequence of turns, but she still had the idea of other paths leading from the entrance to the exit. The fact is also to be noted that early in the series most of the subjects were able to describe *most* of the path—in the stage represented by trails VII-XV.

The essence of the explanation of the irregularity consists in the fact that to each subject the maze was a series of definite and relatively isolated problems, rather than one problem. The number of these special points of difficulty had as a rule dwindled down to one or two after the first VII-XII trails. From that stage on the attention was mainly concerned with these specific regions of trouble, while the rest of the path became almost automatic.

It is evident that the curve does not at any stage represent the development of the subject's knowledge of the maze path. A sudden increase in errors and time may indicate merely that the subject had seen fit to do some exploring, at the sacrifice of voluntarily playing havoc with his record: M. R. F., XXI; J. J. T., VIII; G. M. F., VI. Sometimes these explorations were productive of good results, sometimes not. Again, a sudden jump upwards of the time and error curves was the result of careless accident—the subject through lack of attention would slip into a cul-de-sac, even when he knew exactly the location of its entrance. Once in, he would get temporarily confused, and would escape often only after a trial and error expedient. An example of this is J. W. H. In this and in other cases, the attention had been uniformly concerned with the trial, but had suffered a temporary relapse. In other instances, a bad record was the result of general laxity of attention throughout an entire trial, or its direction to some extraneous affair.

The difficulty of the correlation of efficiency as measured by the total time involved in learning the maze, and the total number of errors made, is obvious from a glance at the following table. In the first place, this objective standard of efficiency is ambiguous. Time and error records represent only two of various possible criteria. The total amount of distance tra-

versed in the maze is a possible standard. Again, there are different methods of computing errors.

The total of the time and errors expended by each subject, in the order of their increasing excellence, is as follows:

Time		Errors	
M. H. S. H.	53′-58″	M. H. S. H.	644
J. J. T.	40′-14″	J. J. T.	412
G. M. F.	32′-32″	E. C. P.	285
H. F. A.	27′-58″	M. R. F.	279
M. R F.	27′-58″	J. W. H.	243
J. W. H.	22′-47″	H. F. A.	213
E. C. P.	21′-22″	G. M. F.	179

The relative predominance of a single specific activity involved in the learning process can hardly be called upon to explain the above lists of the subjects in their increasing order of merit, as the summarized data on these activities show:

Imagery: (a) E. C. P. employed visual imagery to the greatest extent, J. W. H. ranks next to her in this respect, and M. R. F. was a rather distant third. This would seem to suggest correlation between imagery and efficiency. But the two last used visual material in connection with other forms, and H. F. A., whose time record is very close to that of M. R. F., reports no visual imagery at all. The error column fails entirely to suggest a corresponding relationship between the use of visual material and success with the maze. (b) M. H. S. H. and H. F. A. represent the most exclusive emphasis upon motor processes; G. M. F. employed the same type of imagery, but complicated with verbal material. No obvious correlation in this respect is suggested. (c) It does not appear that a combination of image processes, such as was represented by M. R. F., G. M. F., and J. W. H., is to be directly related with skill in the maze.

Thinking and Attention: (a) M. R. F., who attempted to the greatest extent a rationalizing attitude, occupies middle ground in the matter of efficiency. It would be exceedingly difficult to determine how the remaining subjects are to be ranked as to the amount of thinking they did. M. H. S. H. has a considerable amount to her credit, but it was concerned with one particular segment of the maze. (b) On the basis of the

amount of attention paid to the maze, irrespective of the attempt at intellectualizing, E. C. P., J. W. H., G. M. F., and M. R. F. might be listed as representing the maximum amount, with M. H. S. H. and H. F. A. in an intermediate position ,and J. J. T. rather definitely employing the least. Again, assuming that this classification is adequate, it corresponds to the order of merit column only in a general way.

Habit: J. W. H. and M. H. S. H. emphasized the maximum amount as regards this factor, and with them H. F. A. should probably be placed. M. R. F. and J. J. T. report it to a considerable less extent; E. C. P., little or none. A correlation in this respect is again difficult inasmuch as this factor seemed to be detrimental as well as beneficial.

B. SOME EXPERIMENTS WITH MODIFIED CONDITIONS

At the conclusion of the experiment just described, it was thought advisable to supplement the study with a series of additional tests, designed, (1) to serve as a control over the data given by the experiment; (2) to bring out if possible any new data, or to enable us to emphasize aspects of the old. The instructions given for Experiment I were followed, subject to changes imposed by the modified conditions. The tests followed Experiment I in the order described.

1. Description of the tests, and results

Test 1. The subject was asked to go through a maze, the exact duplicate of the one just learned, but reduced in size to one-fourth the proportions.

Each subject made two trials in this smaller maze. Only one of them, E. C. P., made errors in the first trial. She made two, quite obviously due to the fact that her pencil became caught in one of the passages. A few errors were made by two subjects in the second trial, all of which were accidental in their nature, due to the more difficult technique as compared with that involved in going through the larger maze.

The introspections were unanimous to the effect that the con-

trols were the same. The subject, after he had traversed the first passage, formed an estimate of the relative lengths of the other passages, and went through the route correctly, with the errors noted. The various reports read: "Rather uncomfortable, but decided feeling of familiarity of the situation—used same control I used for the regular maze—outside of the novelty of the performance, the same factors were involved." All of them who reported the habit factor in the large maze, testified that it behaved in practically the same manner in this case.

The image controls were simply modified to meet the situation. E. C. P. says: "Used same visual image, *reduced* in size." J. W. H. gives practically the same report. G. M. F. used her usual verbal formula. The subjects employing motor controls report nothing new. They were simply adapted to a smaller objective situation. The habit element was distinctly functional. The increased mechanical difficulty of the operation however made the customary reliance upon it impossible.

Practically every subject, at the close of this test, was positive that he would be able to walk through a maze of the same pattern, blindfolded, with few or no errors. We append this as an indication of the introspective reaction of the subject on the modified condition.

Test 2. The subject was required to go through the normal maze with the pencil attached to the wrist instead of being held by the hand. For this test the pencil was firmly attached to a splint, which was bound firmly but comfortably to the wrist and fore-arm by surgeon's bandage. He was then asked to go through the reduced maze, used in test 1, with the pencil held in the hand, using finger and wrist movement only, the arm being held stationary on a support provided. This second part of the test required one shift of the arm, after the middle of the maze had been reached.

The idea in this test was to eliminate, in the first part of it, the kinaesthetic and tactual factors from the arm and fingers; in the second part, to cut out the same elements from the arm, as far as possible. Since test 1 had shown the same controls to be operative in the smaller maze as in the larger, we felt justified

in employing it for the second part of test 2, inasmuch as the task of going through the larger maze under the same conditions would have necessitated some half-dozen or more arm shifts.

About half the subjects made from 1-4 errors in the test with the wrist attachment. In order of importance, the causes assigned by the subjects were: (a) distraction of the attention by the technique of the test, (b) actual physical difficulty, (c) orientation: this was more difficult under these conditions, as the tactual cues which ordinarily told the subject his location were "blunted." They employed the projected tactual sensibility as best they could. J. W. H. reported that the tactual feeling was transferred, not eliminated.

Fewer errors were made when the smaller maze was run by hand movement only. The main difficulty reported was the sheer physical one.

It is noteworthy that while the subjects in Experiment I consistently emphasized the arm sensation factor more than the hand, more trouble was experienced in the test in which hand and finger sensations were approximately dispensed with, than in the hand movement test. In either case, however, the main emphasis was upon the mechanical difficulty of the process. The subjects were sure they did not, in the first part of the test, elicit hand and finger sensations from the hand, by incipient movements, and then transfer them. They simply utilized the sensations from the wrist instead. In a similar way, they did not evoke arm sensations or images in the second part of the experiment and substitute them for the same sensation normally called out by the actual use of the arm, but they transferred their attention to the fingers and hand, and utilized the sensations coming from them.

Test 3. In this test the larger maze was used, and the conditions were the same, except that the subjects were asked to use the left hand instead of the right, which was used by all learners in Experiment I.

All the subjects reported the process difficult because awkward, but all of them reported the some controls used. J. W. H., who is extremely right handed, reported an actual transference of

kinaesthetic imagery from the right hand and arm to the left. M. R. F. reports it "A transference, not a new learning process. It seemed queer, like the small maze." E. C. P. ran up 16 errors with the left hand in the first trial. She ascribed it however to sheer awkwardness, "the difficulty of doing anything with the left hand." She made no errors in the second trial. M. H. S. H., who depended so exclusively upon kinaesthetic cues and imagery, reported the process the same; it was essentially a transference. In all cases, the transference was made without special effort.

Test 4. Preliminary to this, and to all succeeding tests, the subject first went through the maze, normal running, in order to keep conditions as nearly as possible the same as they were at the completion of Experiment 1.

In test 4 the subject was seated in front of the standard maze, the usual conditions being observed. Instead of allowing him to begin at the starting box, however, the experimenter placed the pencil, upon which the subject retained his hold, in various places in the maze, both (a) in the true path, and (b) in cul-de-sacs. The subject was asked to proceed from these places to the exit, under the condition of a normal trial in the maze. At least 50 different tests were made for each subject, ten or a dozen being made at each sitting. The order was made as irregular as possible, to obviate any attempt of the subject to guess where his next place of starting would be.

The results from the subjects in this test were strikingly uniform. In every case, without any suggestions on the part of the experimenter, and without communication between the subjects, they made a definite judgment, after being placed in the maze, and before they started for the exit, as to their location. After this tendency was observed by the experimenter, they were asked to make their judgments aloud. The judgments were in every case based upon the cues furnished by the muscular and strain sensations from the arm.

In every case the success of the subject in going from the place of starting to the exit with a minimum of errors seemed almost directly proportionate to the accuracy of the judgment. In many

cases he made more errors when placed in the true path than when set down in cul-de-sacs—this, too, in view of the fact that no subject at this time could describe all of the cul-de-sacs, while all of them had a rather definite knowledge of the true path. In one trial that resulted in 15 errors, M. R. F. was set down just before the end of B, but made the judgment that she was at the beginning of G. She started left, was stopped after a short distance, turned up, and was stopped again. So far her experience fully substantiated her judgment. But when she attempted to turn left again (thinking she was on H), she found herself blocked. She reported a sudden and intense sense of absolute confusion, a realization that she was hopelessly lost: "If I'm not *here,* I haven't the slightest idea *where* I am."

This is a very characteristic description of the reaction that Test 4 elicited. If the judgment were inaccurate, the process was one of a trial and error moving around, trying to fit one idea after another to the cues furnished by the experience. Recognition or orientation might come after one or two turns, or might not come until after prolonged exploration.

As the tests were continued, all the subjects increased in their ability to form more accurate judgments. It was practically impossible for the experimenter to compare the relative ability of the different subjects, either at the beginning or at the close of this test, on account of the fluctuations, which were rather extreme.

Test 5. In this test the normal maze was used with the following modifications:

(1) The subject, after being blindfolded, was walked around the room in various directions before being seated.

(2) The chair, table, and maze, were rotated at various angles, 45°, 90°, 180°, etc., from their original position, in an irregular order, while their relative positions remained unaltered. The subject was then seated directly, and asked to go through the maze as usual.

(3) The chair was placed in different positions facing the maze, which, with the table, remained unaltered.

(4) The chair and table remained in their normal position,

but the maze itself was rotated, clock-wise and counter-clockwise, at all practicable angles.

(5) By means of a special apparatus, the maze was made to rotate, in either direction, *while* the subject was attempting to trace through it with the pencil. In all the other conditions followed in this test, no part of the apparatus was changed after the subject actually began the trial. In part (4), the maze rested upon a wood base instead of upon the plate glass, on the bottom of which three wheels were attached. These rested upon a circular track of wire laid upon another wood base, so that maze and upper base could be rotated in either direction, at any speed, by the hand of the experimenter. The apparatus worked smoothly, and was practically noiseless.

In general, very little disturbance resulted from the modifications imposed in the first four series of test 5. For (1) and (2) the subjects were unanimous in reporting that they were not in the least disturbed, that positive orientation did not bother them. All of them had previously been aware in what direction the chair faced. No amount of turning them around, in (1), enabled the experimenter to disturb their sense of position. No errors or increase of time resulted from (1) and (2).

The introspections from (3) and (4) were similar. The image controls were simply adapted to the new situation. The process, on the part of J. W. H., seemed to accentuate his imagery. E. C. P. said that her visual image was "turned around" to meet the new situation. With subjects who had emphasized kinaesthetic and tactual factors, the new conditions called out nothing new in these processes, notwithstanding the fact that for any turn or path of the maze, an actual different set of muscles might be involved from those used normally. There was with all a consciousness of a new arm position, and a consequent feeling of awkwardness, but the process, in this as in Experiment I, was one of controlling a certain sequence of turns. The kinaesthesis was built up in terms of this sequence. For tests (3) and (4) we recorded a few errors that were ascribed to general distraction of attention.

The rotating maze was, in the following order, (a) turned

slowly, at a uniform rate, in one direction, without any instructions being previously given to the subject; (b) rotated faster, at the rate of about one revolution per 10 seconds (the average time it took the subject to go from entrance to exit under normal conditions) in either direction, but constant for each trial, until the exit was reached; (c) rotated, during any one trial, in either direction, with frequent and variable changes of speed and direction.

The observation of the behavior of the subject when he first tried the rotating maze proved to be extremely interesting. The maze was moved very slowly. The subject covered about one-fourth of the path before he realized that something was wrong. Most of them became vaguely aware that something was unusual, and thought that the angles were being altered. Very few errors were made, however, during this trial.

In the series in which the maze was rotated slowly in one direction and at a uniform speed, the introspections were very similar to those given in (3) and (4) of this test. The subject first got the cue of the direction and speed of rotation from the sensations of the arm as he traversed the path A-B. He then tried to gauge his movements in terms of the new conditions. The visualizers testified that their visual image "rotated," keeping pace, and checked up by, the maze itself. They modified this statement to the effect that they visualized the pattern in a number of successive positions, which followed in general the rotation of the maze. Those employing other controls reported nothing not given in (2) and (3). All of them proceeded slowly and cautiously and very few errors were made.

In the last part of this test, in which the maze was rotated with all possible variations in change of speed and direction, the task of tracing through it proved to be either extremely difficult or flatly impossible. The subject could not get adequate cues as to the changes that were being made, or if he could, he was not able to control his movements quickly enough to get a new orientation before a new change was made. He was able to keep in mind rather clearly what path he had reached, but he was not able to proceed from there. If, e.g., he had reached the middle

of L, a few rapid turns would suffice to confuse his sense of direction, and he would not continue from sheer inability to tell whether he was headed for K or M.

Test 6. In this test the subjects were started at the exit, and instructed to proceed to the entrance. Otherwise, the conditions were the same as for Experiment I.

The task was not found difficult. Two subjects made errors the first trial, but corrected them easily. There was the tendency in evidence that characterized all the tests to accentuate the imagery employed. The introspections were in most cases practically a description of the objective behavior: they simply followed the path backwards. The motor subjects relied a little more on prompting by verbal cues, but the same kind of anticipatory motor imagery was reported. The habit factor was at a minimum for all the subjects, but after the test had been repeated several times it began to make its appearance at a relatively earlier stage than it did in the first experiment. In general, the trip was made by each subject reversing the various steps in his idea of the sequence of turns, in terms of the imagery he employed for every test. The usual kinaesthetic and tactual sensational cues were attended to for the purpose of checking up the control idea.

Test 7. The maze was turned upper surface down, and clamped to the plate glass as usual. Since the grooves were cut through the board, this reversal resulted in the "mirror" maze. The subject was told of the alteration before he made the trials.

Most of the subjects expressed surprise that the first trial did not prove more difficult. Some found it as easy as the backward running. The same introspections were given: an adaptation of the usual control imagery to the new situation, as checked up by the experience of actually going through the path and attending to the sensory recognition element. M. R. F. who used some visual imagery in test 6, as she had in Experiment I, reported herself as unable to visualize segments of the mirror maze, and consequently she relied on verbal and kinaesthetic cues. She found the process no more difficult however than that involved in test 6. J. W. H. and E. C. P. reconstructed their visual

images, making them "mirror" in form, to suit the condition. They reported them just as vivid as ever, equally stable, equally *adequate* for the purpose they served. The objective records for tests 6 and 7 were strikingly similar.

Test 8. This was in reality an elaborate series of tests designed as objective checks on imagery. The subjects were required to read aloud, to repeat different jingles in terms of auditory imagery, to follow a visual diagram perceptually, to following a moving light with the eyes, while tracing through the maze. They were given various tasks to do with the left hand while using the right in going through the path. Tests to eliminate and to control eye movement were introduced.

Our tests elicited the results characteristic of many tests for imagery: they practically amounted to general distraction tests. The tests were continued in the hope that even if they were of this nature, certain relative results would be obtained that would bring out the desired factors. That is, it was assumed that if any one of the tests were continued indefinitely, it would prove to be relatively *more* of a distraction test for the imagery aimed at than it would be for other forms of image processes. We were disappointed in this respect.

The results from the auditory-verbal tests, in the case of G. M. F., were relatively satisfactory. This subject was made to go through a maze for a number of trials, at the same time repeating "Mary had a little lamb," continuously to herself. She testified that it interfered seriously with her verbal formula. She was forced to "slip in" her verbal directions, between the words of the verse she was asked to repeat. The result was no errors, but a perceptible increase in time.

The visual distraction tests were productive of no positive results. They simply interfered with the attention involved in going through the maze. With E. C. P. the verbal distraction tests disturbed her visual imagery fully as much as did the visual tests.

Certainly, however, these tests offered no possible basis for challenging the introspections of any subject on the matter of imagery.

In connection with this series, three small and comparatively simple mazes were learned by each of the subjects by three respective methods of presentation of the true paths. (1) For the first maze the subject was required to memorize before starting a verbal set of directions which were typewritten and handed to him. They included directions for the true path, not for the cul-de-sacs, and began as follows: "¾ inch up—4 inches to right—4½ inches to left—1 inch down," etc. The subject was required to memorize this accurately before he was allowed to start in the maze itself. (2) In the second case, the subject studied a visual pattern of the true path, from which the cul-de-sacs were omitted, drawn on paper, of the exact size of the path itself. (3) For this presentation, the blindfolded subject traced through the maze to be run with entrances to all cul-de-sacs blocked.

The subject was asked to retain his hold on the maze in terms of the imagery used in the presentation of the path. He was not allowed a trial until he had satisfied the experimenter that he knew the path he was about to trace.

No subject was able to comply with the instructions as to the manner in which the path was to be retained in memory. E. C. P. studied the visual pattern as drawn and remembered it in the appropriate imagery. She also succeded in committing to memory the verbal formula in verbal terms, but was unable to refrain from translating this into a visual image when started in the maze. G. M. F. and M. H. S. H. were unable to remember the visual drawing as a visual image, but reinforced the scant image they got by verbal comment and kinaesthetic aids, before they attempted the maze itself. These three tests convinced the subjects that they had not been in error in their respective reports as to the imagery they normally employed. As for the objective results, the learning of the verbal set of directions proved to be by far the most difficult and irksome task they were asked to do. The learning by visual presentation was much easier for all, the learning by actually traversing the maze was accomplished with the greatest facility by all. There was little or no evidence of any correlation between the method of presentation and the

image technique of the respective subjects as to relative ease in learning.

2. Significance of the results obtained from tests

The data briefly given represent a summary of the evidence contributed by the tests. It substantiates the introspective testimony to the effect that the control of the maze learning process was largely an ideational matter. The significant thing, for instance, as brought out in the "mirror" test, was the ability of the subject to adapt his visual or other image, to manipulate it, rather than the fact that his idea was expressed in certain structural terms. Little evidence offered itself to indicate that the individual mental processes of any subject were relatively more adequate for some of the conditions than for others.

C. EXPERIMENT II

While the present investigation was concerned primarily with the activities of the human subject in the pencil maze, as a study of a definite learning process, it was thought desirable to introduce into the experimental work a maze through which the subject actually walked. The object was to determine whether or not different learning processes were involved in the two mazes calling for different kinds of physical technique. In other respects, the conditions were kept the same. The subject was blindfolded, was given practically the same directions used before.

1. Description of maze, and method

(*a*) *The maze:* The experimenter was saved the immense amount of time and labor necessarily involved in the construction of such a maze by the offer of one admirably adapted for the purpose of the experiment located in Forest Park, Chicago. This is one of the amusement parks of the city, and the maze, called the "Mouse-trap," had been used as a pleasure devise for several years. None of our subjects had seen it or heard of it.

A diagram of this maze [Fig. 4] is given on the opposite page. It was duodecagonal in shape, with the various paths arranged in concentric fashion, leading to the exit in the centre.

From the exit the subject ascended a circular stairway, and by means of a boardwalk on top of the maze, extending from the center on one of the radii and leading to a stairway outside the maze, he descended, thus obviating the necessity of returning through the maze.

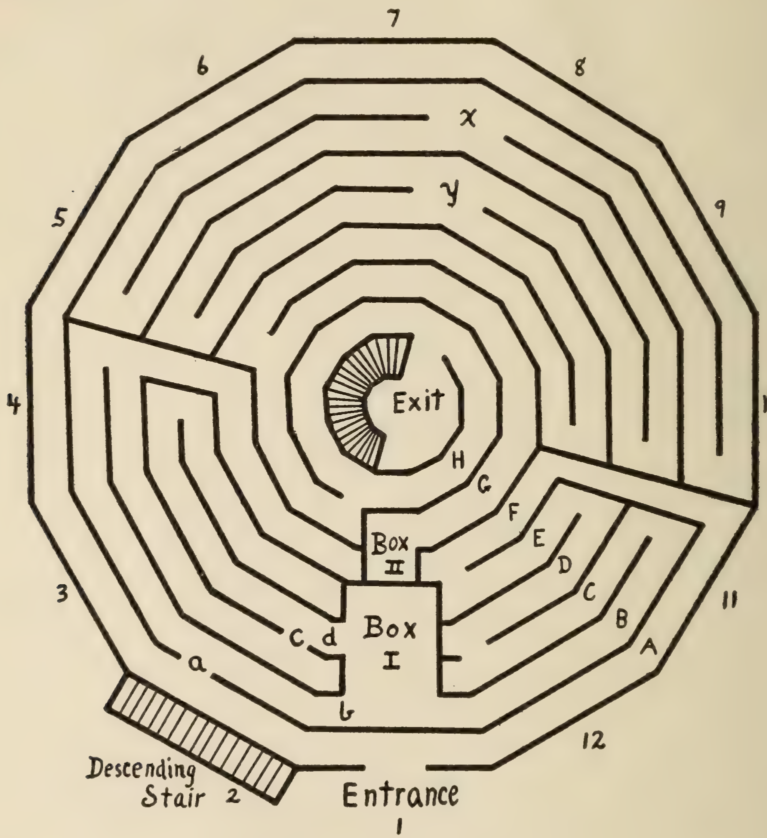

FIGURE 4. The "Mouse-trap."

The "Mouse-trap" was constructed of sections made of wire netting, bordered and held together by angle-iron. The wire was of uniform size, about a 12-gauge, woven into a diagonal 1½ inch mesh. The maze rested upon an even wood floor, elevated about 6 inches from the ground. There were as many sections, in any one partition, or in the outside wall, as there were sides of the duodecagon around which the partitions extended.

The maze was covered by the wire netting, divided into 12 sections, each section corresponding to a sector of the top. From floor to top the height was 7 feet; each alley was of uniform width, 2 feet 4 inches. The length of the sections making up the outside wall was 12 feet 2 inches, so that the circumference of the maze measured 146 feet. The exit box in the center, from which the stairway ascended, had a diameter of 9 feet 4 inches. All openings or doors were of the same width as the alleys. Two box-like areas were located, as is seen in the diagram, in section 1 of the maze. One of these, which is referred to as Box 1, was 8 feet in width, 9⅓ feet in depth; the second enclosure, Box 2, was of the same width, and was 4 feet 8 inches in depth.

The maze was in the open, and side 1, on which was the entrance, faced the north. The floor was perfectly level.

(*b*) *Method of conducting the experiment:* The subject, after entering the park, was led directly to a small building about 60 feet northeast of the maze, conveniently situated so that he did not see any part of it. There he was blindfolded as in Experiment I, and led to the entrance. He stepped upon the platform, rested his two hands on the two sides of the entrance door, and waited until the experimenter mounted the overhead walk and gave the signal, "start."

The directions and instructions given in the preceding experiment were modified to the following extent: The subject is permitted to use both hands as he wishes, getting with them all the tactual cues possible. (This allowed him to feel the floor if he so desired, but only one subject resorted to this, in one trial, and he soon gave it up as useless.) He was permitted to walk forward or backwards, run, or carry himself in general as he saw fit. The maze was considered learned when the subject had gone by the shortest route from entrance to exit three times in succession without error. The mention of more than one possible path was made for this experiment in the same manner that it was in the previous one. In this case, however, there was an actual option of paths.

The experimenter stood quietly upon the platform and wrote down, in addition to the time, a literal account of the route each

subject followed for each trial. This was not an especially difficult task, as the actual time involved in walking gave the observer ample opportunity to take complete notes of everything he desired to record. The experimenter was careful, except in one or two exigencies, to say nothing to the subject, who, however, was encouraged to express his introspections or comments aloud, to any extent which did not act as a distraction. The subject was told absolutely nothing of the plan or construction of the "Mouse-trap." It was simply "a maze," which he was to learn by walking through it. When the learner reached the exit box at the end of the first trial, he was told to "stop." In the ensuing trials, he had no difficulty in recognizing it. He was led, still blindfolded, over the walk, down to the ground, and back to the building mentioned, where the bandage was removed and the introspections were called for.

Owing to the distance of Forest Park from the psychological laboratory, it was practically necessary for the subject to make the trials in succession, with intervals of 10-15 minutes for rest and introspections. Each subject spent a good half day at the performance; two of them made two trips before they learned the maze. The experiment was conducted in October, 1911, and the following were induced to act as subjects: J. R. A., M. R. F., W. S. H., E. W. B., R. B. O.

2. The objective behavior of the subjects in the maze, and the nature of the learning process

(*a*) *General description of behavior:* There were very few individual differences to be noticed in the behavior of the subjects. They proceeded very cautiously, especially for the first few trials. Both hands were employed in feeling the sides of the alley ahead for the openings. Several times during the first or second trial the subjects became so engrossed in the search for openings that they ignored the possibility of blind endings in the path, and a few unlooked for bodily contacts with the ends of blind passages resulted. This happened a few times in the later trials, as the result of over-confidence in their ability to guage the lengths of various passages. As they learned the path,

the speed was increased in familiar regions from a blind man's walk to a brisk half-walk and half-trot. Most of the learners, when they had reached openings, would stop for the purpose of studying or attempting to remember their location. When either of the two boxes were entered, the subject would as a rule keep one hand in contact with the side, for the purpose of retaining his orientation, and reach as far out as possible with the other. No subject attempted walking backwards, or any other pronounced variation from the regular procedure. They would occasionally stop and face in different directions, in an attempt to get a better orientation.

One subject, J. R. A., in the absence of directions to the contrary, wore gloves in practically every trial. Several of the subjects at different times availed themselves of this protection from the metal. As will be mentioned below, this did not interfere with the tactual discrimination employed.

(*b*) *Nature of the learning method:* The variation in method in evidence in the pencil maze was not so pronounced in this experiment. The introspections were more similar to those of M. R. F. in the normal maze. They indicate a more persistent attempt at studying out situations. No subject reported complete reliance on the hit-or-miss method of H. F. A. The fact that the subject had more time to think or plan in the "Mouse-trap" partly acounts for this, and the reports indicate that the greater complexity of the maze itself called for more intensive study. There was however a rather sharp fluctuation between periods of study and directed exploration and periods of aimless trial and error with four of the five subjects in the first trial.

(*c*) *A report by trials of the learning behavior of one of the subjects, and a brief description of the learning of the others:* In the following account, references to segments of the maze are abbreviated as much as possible. Thus, 2-3 refers to the general segment corresponding to the first three sectors of the maze to the left of the entrance, while 2-4 on B indicates one path in that segment. The designations given in the drawing of the maze are followed in this description. The record of R. B. O. was selected as the one best suited to give the reader an adequate

description of the type of learning activity elicited in the "Mous-trap, because this subject had the most difficulty with the maze, and the steps in his learning were more elaborated. The reports of the other subjects had implied in them the same activities found here.

(1) Subject R. B. O.: Trial (I). This subject occupied 1 hour, 15′, and 47″ in finding the exit to the "Mouse-trap," and at the sacrifice of 112 errors, in his first trial. His exploration was divided up as follows: (a) In the first attempt he turned right and entered 11-12, which, with the 2-4 section through Box 1, he went through several times. (b) He started left from the entrance and explored section 5-10 at length, but worked back to the entrance before getting further on the true path than E. (c) The third attempt was largely a repetition of (a). (d) Again he followed A around to 10, failed to find the exit, and ended up in Box 1. This was followed by a prolonged entanglement in sections 2-4 and 11-12, during which Box 1 was entered several times. (e) The exit was finally gained in a last effort in which the whole region 5-10 was worked over in detail.

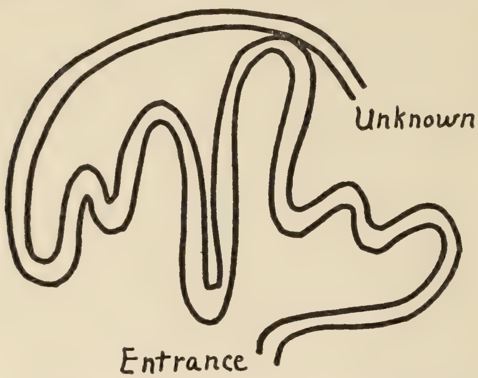

FIGURE 5. Drawing by R. B. O., end of first trial. Figures 5-10 are exact reproductions of drawings made by the subjects.

The report, like the behavior, followed M. R. F. in her first trial. He successively alternated between trying to keep his bearings as he went, and giving himself up in an aimless fashion to whatever he chanced to find. This variation of working methods did not carry with it any perceptible change in objective behavior, but the subject kept the experimenter informed of his fluctuations by such comments as: "Now I'm not going to think for a while," and "Guess I'll study this region." Like M. R. F., his knowledge of the maze acquired as the result of the first trial was vague and confused. [See Figure 5.] Like J. R. A., referred to later, he had conceived the exit to be on the outside and was controlled largely by the general idea of working in that direction.

Trial (II). In his second trial, the subject again started to

the left, came out through Box 1 to A, which he followed to 10. He called A a circle. He became confused by the doors x and y, and found himself back at the entrance. A second attempt was largely a repetition of the first, but resulted eventually in his gaining the exit. Time 17″-24″, errors 31.

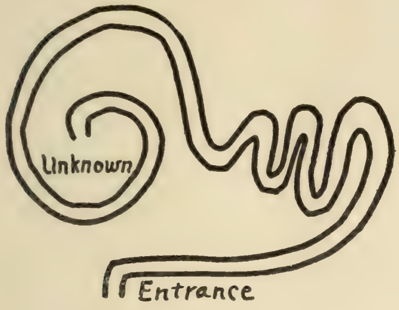

FIGURE 6. End of trial 2.

The main idea the subject got from this trial was the circular nature of the paths, indicated in his drawing. [Figure 6.] Several places were recognized as familiar. He was very much bothered by the two doors x and y, and could not figure out what connections they made.

Trial (III). After going through the region 11-12, through Box 1, and gaining the entrance by way of a, the subject gave the information aloud that he had learned something: "If you go straight across the Box (i.e., along B) you come out at the entrance. Therefore avoid that path." In the obvious attempt to profit by this discovery, R. B. O. after entering the Box again by his circuitous route, searched around for other exits. This led him into D, and the region 2-4 was thoroughly explored in an attempt to find the circular path. This process was repeated, until in desperation he decided to retrace from the Box to the entrance, and then go where "instinct" would guide him. As a result of this retracing, he got started on A, followed it around past the entrance to 10, and finally made the exit.

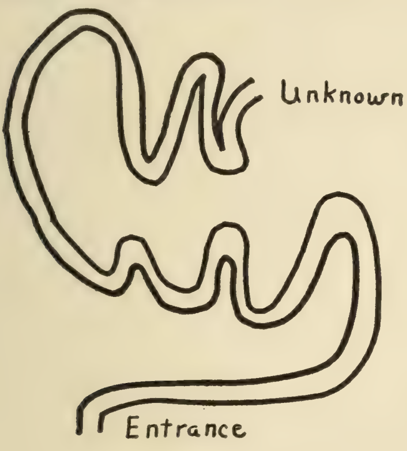

FIGURE 7. End of trial 3.

R. B. O. describes this trial as "pretty much hit or miss." He doubts if it added to his knowledge of the path—he rather thought he knew less than at the end trial (II). [See Figure 7.]

Trial (IV). The subject did not learn from the last trial's experience that he should start directly to the left. Accordingly

he started on 11-10 and found himself as usual in the Box. As a result of a considerable amount of exploring in 2-4, he decided on door *d* as the correct way out of the open space. In an attempt to act on that conclusion, the subject repeatedly walked back and forth in C D E F, in the region 2-4. After this exploration he concluded that B after all was the correct way to escape from the Box, and after more experimentation he discovered that *b* and *a* led him from the Box to the circular path. Once in the region of the doors *x* and *y*, however, his difficulties were renewed, but he finally reached the exit.

R. B. O. describes this trial as much more systematic than the last two. He did more planning, more rational exploring. He was able to do so because turns and passages were becoming familiar, and he could retain more in memory, and therefore *think* better. He describes the process as one of "exploring, and building up the path." His actual knowledge of the maze, however, as expressed in his diagram, does not differ much from what he had known before. [See Figure 8.]

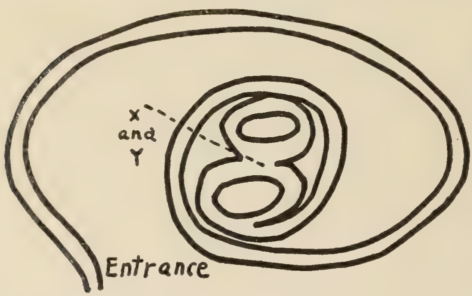

FIGURE 8. R. B. O.'s: first conception of X and Y. End of Trial 7.

Trial (V). In this trial, R. B. O. learned to start to the left directly from the entrance, and thus avoid 11-12. This he calls a purely accidental discovery. He made no headway, however, with the regions concerned with *x* and *y*. No matter what way he turned, he got back to the door *a* again—he did not know whether or not there were two doors. His knowledge of the maze was increased, but every new fact, he stated, was the result of accident: "The best intelligence I have doesn't get me anywhere."

Trial (VI) objectively represents the greatest drop in the learning curve, but it was the result of chance, the subject thought, and did not represent any correlated addition to his conception of the path.

Trial (VII). In this trial the center of study shifted entirely to the doors *x* and *y*. The net result of an exploration in that region he expressed as follows: "Turn any way you want to, at this door, and the chances are that you find yourself back at it after a short time." He repeatedly, after reaching *x* by turning into B from A on 10, would continue on B to 5, come down

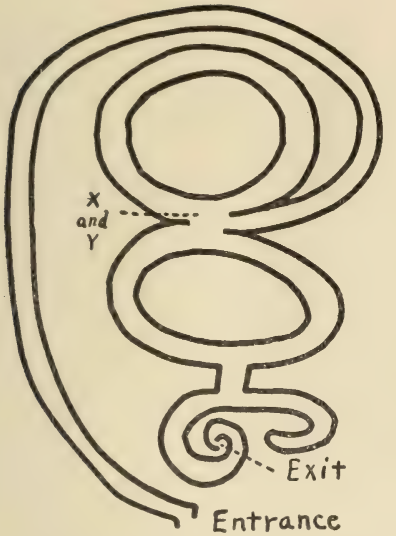

FIGURE 9. Second idea of X and Y. Early part of Trial 10.

C and find himself at the same door again; or would turn correctly at x, come back on D, and again find what he thought was the same door. He studied long on this problem, but was unable to figure out what possible arrangement of paths and doors could result in such an experience. The first explanation that he gave was that the maze was a figure 8, and that the true path intersected at this door. He reports himself at the end of the trial as "Completely baffled—I know less than I did at the beginning."

Trial (VIII). While this trial was made with only three errors, he made no progress in the difficulty of the last trial, and still believed the maze was a figure 8. [See Figure 9.]

Trial (IX) was a repetition of (VIII).

Trial (X). The intellectualizing in this trial consisted in a prolonged attempt to imagine different spatial possibilities, and explain his experience in terms of them, of the region in the vicinity of the two doors. The diagrams drawn by the subject represent his three conceptions of the region, in the order of their elaboration. [Figure 10.]

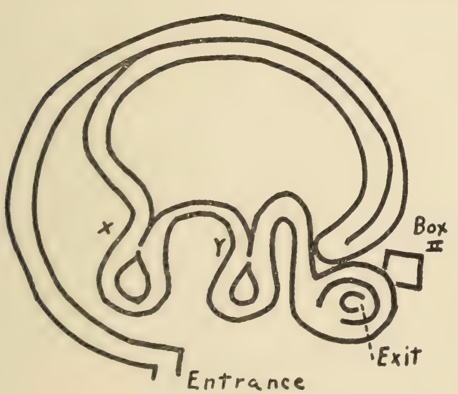

FIGURE 10. Final idea of X and Y. Trial 10.

Trials (XI-XII-XIII). No errors were made in these trials and the maze was called learned. Since the explanation he had last elaborated *worked,* he "let it go at that" as he said in the last trial, and concerned himself only with the task of making the right turns.

(2) Subjects E. W. B. and W. S. H.

The records for E. W. B. and W. S. H. were similar to those of the subject just described, but indicate less trouble with the maze. Neither of them had the difficulty with the doors x and y that was the source of R. B. O.'s confusion. The drawings they made after each trial are quite comparable with those reproduced.

The learning of W. S. H. offered one peculiarity, inasmuch as he was unable to learn the maze technically, in the trials he made because of the fact that he did not discover the shortest path. This subject, at the end of trial (XIII) reported the maze "learned"—that is, he had gone over what he considered the true path three times without error. He had, however, incorporated the section 11-12 into his route, and hit the true path after going through Box 1 and door a. He had not doubted for an instant that this section was part of his regular course.

Since the experimenter had asked for the shortest path, he informed the subject that he had not fulfilled the conditions of the experiment in this respect. The statement was made that there might be an absolutely new path, or that his error consisted in not discovering one or more short cuts in the path learned. The subject then started on a new trial.

In this new trial W. S. H. started directly to the right several times, and attempted to discover a short cut from Box 1. After each attempt he followed the old path, and worked in 5-10 for a shorter route in that region. Curiously enough, in one attempt he started directly to the left and followed the shortest path to x, but he got confused in that section, retraced, and found himself back in Box 1 again. On the basis of this experience he dropped the idea of starting directly to the left. After two more trials in which no more headway was made, the lateness of the hour prohibited additional experimentation, and the maze was called "not learned."

(3) Among the remaining subjects, M. R. F. had the most difficulty, with E. W. B. as a close competitor, while J. R. A. has the smallest number of errors and the minimum amount of time to his credit. The first trial of M. R. F. occupied 1 hour, 20′,

and 56″, and resulted in 175 errors. The chief landmarks she discovered in the early part of the trial were the "box," and path A, which she described as "some considerable curve." As in the case of R. B. O., the doors x and y were the occasion of much wonder and not a little disgust. The first trial was characterized by sharp alterations between the methods of trial and error, and attempted systematization of her attempts. On the whole, her learning seemed to follow the procedure shown by R. B. O. The efforts of E. W. B. in their essential aspects were in turn very similar to those of R. B. O. and M. R. F. J. R. A. started in with the preconceived scheme to follow consistently the right side of the path. This led him in his first attempt almost directly to Box 2, but it also led him back to the entrance from that place. Although conscious that the path seemed to be a kind of narrowing spiral, the conception of a side exit was sufficiently strong to shut out any idea that it was in the center. The exit was discovered by pure accident. J. R. A. elaborated a definite verbal formula, which read, beginning at the end of A on 10, "right—left—right—left—right—right—left." With its aid the path was learned successfully in short time.

3. Summary and analysis of some of the specific aspects of the learning

(*a*) *Imagery, and sensory processes:* In general, image and sense material functioned in the same manner that it did in the first experiment, but some differences in the behavior of these processes in the respective mazes were in evidence. The tactual element was emphasized much more strongly, as was to be expected from the fact that in this experiment the hands were actually employed in feeling the sides of the maze. Since, however, the partitions were made of homogeneous material, and the openings were similar in size and construction, the subject did not succeed to any marked extent in recognizing familiar landmarks by tactual cues alone. One subject, R. B. O., searched the floor for irregularities to serve as points of reference, but was not successful in this attempt. The hands and arms were used mainly in informing the subject when doors were reached, and in

keeping him from walking into blind alleys, rather than in making fine discriminations. Hence those who used gloves found that they did not seriously interfere with this tactual function.

The reports on imagery follow closely those made in the pencil maze. Verbal processes seem to have been called upon somewhat more freely, both by way of comment and elaboration of formula. Verbal material was used in two ways (1) in the "building up" process, of discrimination and association—that is, in studying out situations. (2) It was employed to fix landmarks or regions of crisis—that is, the verbal formulas used consisted of directions, associated with particular doors. While R. B. O. *knew* the maze in terms of visual imagery, he had used verbal material in building up this knowledge, in working over confused details. W. S. H. also used verbal material in this way, and consequently reported it as dropping out towards the close. E. W. B. used words to "start" himself—e.g., "Now I'll do so and so."

Visual imagery was reported by W. S. H. to be more accurate than the verbal or kinaesthetic which he also employed. It was the visual scheme which "set" him in the right direction at diverging paths, as over against kinaesthetic, which was more general and vague. But M. R. F. used relatively less visual than she did in the pencil maze.

No subject reported the use of only one type of imagery, as was done by one subject in the pencil maze, W. S. H., who represented a combination, used verbal in some regions, kinaesthetic in others, both mixed with visual. This using of different kinds for different segments was also reported in the preceding experiment.

The slight differences we have noted in the use of imagery may be merely matters of individual variation, or they may be explained by the fact that the trials were longer in time, than in the pencil maze, and allowed more time for studying or memorizing, and as a consequence elicited more verbal material. It was not evident from the introspections that imagery was employed in different ways, although the body itself was in motion in this experiment, while it was a stationary point of reference in the pencil maze experiment.

(*b*) *Habit and Attention:* We did not discover the appearance of habit, in the sense of paths being run unconsciously, to the extent that it was manifested in the pencil maze, nor did the introspections indicate much reliance upon this factor when it did appear. Probably this latter statement is the more significant one. In the pencil maze, the subject made, in any one path, a vigorous sweep with the pencil and was stopped rather violently by the end of the path. The greater complexity of the act of walking in narrow confines, and the fact that sudden contact with the end of a passage involved physical discomfort, was the explanation the subject gave for the greater amount of caution used in the "Mouse-trap."

Again, there was more variation of objective behavior possible in this maze. Any given sequence of turns was actually made proportionately a less number of times. In any cul-de-sac in the pencil maze, the subject who had entered it, was forced to "back" out. Cul-de-sac regions in the "Mouse-trap" were open at either end, such as 11-12, or B C D E in 5-7.

The introspections on the last trials however, disclosed on the part of R. B. O. and W. S. H. a decided tendency to let down on the active attention formerly used—a stage which in the pencil maze was the fore-runner of automaticity.

The attention during the learning was directed in the same manner that it was in the preceding experiment, and but one point of distinction appeared in the reports. As before, it was either retrospective, engaged with present experiences, or anticipatory. In this latter aspect of its behavior, the reports in the pencil maze were to the effect that it was concerned with the turns to come as expressed in some image form. In the park maze, there was a report from M. R. F. that her attention was at times concerned with a more general state of expectancy, or surmising, or guessing. Other reports seemed to indicate a tendency towards this same type of anticipatory attention. As the maze became controlled, the anticipation, as before, was on the turns in front of the subject. The distinction seemed to be a relative one.

(*c*) *Discrimination, Memory and Recognition:* The fact was

mentioned above that the maze was composed of homogeneous material, and that fine tactual discrimination was impossible. Discrimination of the less immediately sensory kind, that involved in ascertaining the nature of the paths and cul-de-sacs, had in it therefore, less of the purely sensory element than the activity in the pencil maze. It was of the kind that called upon the ability of the learner to interpret experiences by thinking, applying concepts.

It was assumed by the experimenter that the subject would in the first trial, as a matter of discrimination and interpretation of direct sensory experiences, gain the idea of the segmental and curved nature of the paths, and would be able to interpret the paths as belonging to a concentric system. Since the subject kept one hand in constant contact with one of the walls, and since the lengths of the segments decreased as he went towards the center, it was thought that the angle of 30° would be sufficient to furnish him ample cue for this conception.

As a matter of fact, all of them did note that the paths were irregular. One other subject, with M. R. F., observed in (II) that the path A had "some considerable curve." They were a long time however in getting the concept of the nature of the paths; and they were decidedly late in hitting upon the idea of the concentric arrangement of the paths. Some of them hardly got the idea at all. The drawings reproduced above sufficiently indicate the tardiness of the subjects in discriminating and interpreting the sensory experiences of the maze.

Inasmuch as discrimination did not differentiate the various paths to a degree that each of them presented distinct peculiarities in curvature and length, the number of regions that early became definitely familiar was small. Box 1 was a landmark for everybody from the start. The doors *x* and *y* were distinct from others doors, but being in themselves alike, they were a source of confusion to all of the subjects. The region 11-12 early was recognized, inasmuch as it was a series of alternating paths without outlets. The region 2-4 was the area of greatest confusion. It was complex in arrangement without offering landmarks, and it was not learned by any subject.

In the pencil maze, the fact that the body was a fixed point of reference aided the process of discrimination and recognition, since it, (1), gave the subject a fairly accurate idea as to what part of the maze, (right, left, upper, etc.) he was in; and, (2), gave the subject an immediate cue as to the direction of any one path. In the "Mouse-trap," the absence of this reference point, together with the fact that any one path was not a straight run, but was composed of segments, and was therefore curved, tended to make these judgments much more difficult. The park maze, presenting as it did, more variations in cul-de-sacs types, instead of offsetting this disadvantage by way of holding out more individualistic segments, resulted in confusion, because of its complexity. The most difficult region in the pencil maze, was the most complex cul-de-sac, 6-9, which was simply an inverted capital T. As compared with the 3-4 region in the park maze, it was exceedingly simple.

Memory in both mazes was obviously employed in two ways. (1) Remembering the path, after it was once learned, was one of the things the subject would be assumed to effect. Practically, this meant, however, the ability to remember crucial turns in the path, where opportunity for error was present. J. J. T. and others, when the pencil maze was learned, were not able to describe the true path, since they had memorized only the important segments of the path. (2) Studying the way out of difficult regions consisted largely of calling up and applying memory experiences.

The second function of memory was relatively put under more strain in the "Mouse-trap" than in the pencil maze. Once they were learned, the two respective paths were remembered with relatively equal ease. While the route in the pencil maze consisted of 32 turns as over against 9 in the other, the important thing was the number of opportunities for error in the two paths. There were 6 cul-de-sacs discharging into the true path in the pencil maze, and 9 in the park maze, but only two of these, *x* and *y*, proved especially difficult to remember.

We did not find that the differences in physical technique in the two mazes influenced the process of memorizing, by way of making it either more or less difficult.

(*d*) *Illusions:* Evidence has already been offered to suggest that, as in the pencil maze, the number of spatial misconceptions was pronounced. The reproduced drawings given above indicate the nature of these illusions or misconceptions.

In another way, this phenomenon was brought out. The subjects were allowed to see the maze after it was learned. Most of them expressed intense surprise at the small size of the "Mouse-trap" when actually seen. For W. S. H. it dwindled down to one-half its size. R. B. O. made the proportions in the same directions, 5 to 1. Other subjects were more numerically conservative in the same judgment with the exception of E. W. B., who reported the maze as larger, not smaller, when actually seen. This subject had made rather accurate estimates of dimensions while learning the maze, but some of the essential ones had been smaller than the segments measured.

(*e*) *Emotion and Affection:* The "Mouse-trap" seemed to elicit the unpleasant reactions more than did the mazes used before. The work was more physically fatiguing, and the feeling of being hopelessly lost was more in evidence. M. R. F., towards the close of the first trial, was once at the point of declaring that she could not learn the maze. There was, however, a corresponding elation when significant discoveries were made. Only in the case of W. S. H. was the feeling of being baffled in evidence towards the close, when he was directed to discover the shorter route. This, with the fatigue that had developed by that time, influenced the course of his learning.

Emotional disturbance, however, played a positive rôle in the learning of this, as well as the other difficult mazes. They represented periods of intense consciousness, in the same way that periods of mental effort meant a heightened consciousness. In either case, this consciousness was called into being when the need of readjustment was imperative. Current psychological doctrine asserts that cognitive activity functions in times of conflict, and while it assumes that emotion also arises under similar conditions, it has not assigned to that state any definite function. That function, in the maze learning process, is indicated in the introspections.

The subject described his efforts, while under the stress of such excitement, as pure trial and error; but they actually represented an entirely different thing from the listless, random exploration that was on other occasions characterized by the same subjects in these terms. In the first place, the subject was more, not less, sensitive to the significance of the attempts he made. He was decidedly on the alert for possibilities. Hence, discoveries made during these periods were utilized and reacted upon as quickly and efficiently as those which were the result of careful study. Secondly, the effort was not directed by any interpretation or theory—they were practically thrown to the winds. As a result, openings were entered that were normally labeled as cul-de-sacs, and avoided. It was the effort of the drowning man to clutch at the last straw; and such an expedient often turned out to be highly successful in the maze. S. M. R. discovered the exit of maze M in such a period during which, out of sheer desperation, she entered the cul-de-sac complex which in her rational moments she had avoided.[3] In general, emotion was the incentive to a more inclusive series of exploratory movements than was the case when these attempts were controlled by ideas. Emotion very often meant increased effort upon new lines, made possible by the discarding of old conceptions and theories; but the value of the reactions was not lost sight of, hence they were often productive of positive results. Obviously, emotion carried to the extent of surrender, to a cessation, rather than an increase, of activity and effort, would defeat its own purpose, as it very nearly did in the two cases mentioned.

(*f*) *Thinking:* The fact that all the subjects who had serious difficulty with the maze alternated frequently in the first trial between periods of active study and periods of relatively aimless trial and error has been mentioned. It indicates that there were definite periods in the subjective aspect of the learning process in which the higher mental activities of the learners were called into play. This fluctuation also characterized the subjects in the pencil maze, possibly to a less extent.

The experimenter assumed, at the beginning of trials, that the

[3] *Cf.* Experiment III.

"Mouse-trap" afforded more opportunity for study, for thinking out situations, than did the pencil maze previously employed. Its cul-de-sac formations were more complex, and a richer variety of experiences, it was thought, would present material for a more complex type of mental reaction.

As a matter of fact, our assumption turned out to be correct inasmuch as the attitude of the four subjects resembled that of M. R. F. in the pencil maze more than it did that of H. F. A. For J. R. A., the maze was learned too easily to elicit that subject's method of thinking out such situations. The quality of the attempts at rationalizing, however, seemed to be quite comparable with those brought out in the previous experiment.

None of the subjects guessed or reasoned that the exit might be in the center, rather than on the outside of the maze. Predilection was against them, it is true, on this point: those who had previously worked with animal mazes were accustomed to the side exit formation. But after J. R. A. had in the first trial followed the true path almost directly to the center, and had discovered rather definitely the concentric nature of the paths, it did not occur to him that the exit was possibly there. Two subjects reported that in the first trial they were definitely working for the outside. They did not question the presumption that the exit would be here.

As in the pencil maze, and to a greater extent, general working ideas were in evidence—definite rational methods of attack. Thus J. R. A. and M. R. F. resorted to counting their steps in specific places where estimations of lengths were desired. The scheme to follow all turns to the right, or to the left, the plan of R. B. O., (IV), to locate various central points of reference, and work out in all directions from these, and even the deliberate adoption of a random hit or miss method are examples of the general control ideas—ideas that were the result of definite judgment to the effect that they might prove efficient.

The actual efficiency of these general methods was extremely hard to determine. J. R. A. hit upon a plan that resulted in speedy learning. M. R. F. conceived the same idea, but it did not work for her. She had also, in Experiment I, in the corre-

sponding trial (I), attempted and dropped this working scheme.

The extended study R. B. O. made of the doors *x* and *y* has already been described. It seemed in every way comparable with the study made by M. H. S. H. of the cul-de-sac 6-9 in the pencil maze. It certainly involved the mechanism of the reasoning processes: it was something more than sensory discrimination, or imagining, or memorizing. But the actual solution was an accidental discovery.

The difficulty of labeling the type of mental activity in evidence was as obvious in this experiment as in the previous one. W. S. H., after being informed that he had not discovered the shortest path, actually did go directly from entrance to door *x* in his endeavor to find the short cut. His attitude was one of sharp attention—he was on the lookout for cues, he was doing all the thinking he could do. But with all that effort, he did not interpret sensory experiences in an adequate way. Systematic thinking seemed to be extremely difficult, or impossible.

4. The objective results

The objective records, presented in the form of curves, show a relatively different distribution of time and error from that of the pencil maze. A greater proportion of effort was centered in the first trial, in this experiment, and the ensuing trials are more free from irregularities.

The introspections, and the fact that the records from the pencil maze employed in Experiment III are comparable with the results from the "Mouse-trap" in this respect, indicate that the cause of the relative difference in the distribution of effort is to be looked for in the plan of the maze, rather than in the fact that it was a maze calling for a different physical technique, or other factors.

The reports on the rôle of discrimination and memory explain the relatively greater emphasis on the difficulties of the first trial. By far the most difficult part of the maze was the half on the entrance side. This was practically one system of cul-de-sacs, since only one door opened into it from the true path, in addition to the option offered at the entrance. An immense

amount of exploration was necessary for the subject to learn *not* to enter door *a* and *not* to start to the right. These items, once learned, were easily remembered, while the actual scheme of these cul-de-sacs was speedily dropped from memory. On the basis of similar records from Experiment III we might assume that a maze of this type would tend to throw the emphasis on the first trial, while a maze consisting of a long true path, with a number of cul-de-sacs distributed along its course, would give a curve characterized by a less pronounced initial decline, and a more uniform distribution of irregularities.

As in the preceding experiment, it was obviously not easy to correlate efficiency with any one abstracted activity. Some of the simplest mental operations seemed to have involved in them a rather complex type of mental organization.

D. EXPERIMENT III

The results of Experiments I and II had indicated and emphasized an apparently paradoxical behavior of the rational aspect of the learning process. The fact had been made evident in every introspection that the subject employed in his learning, not a number of isolated activities, such as imagery, discrimination, and others, but a more essentially complex type of reaction. Sensory discrimination had involved in it, for instance, imaginal and conceptional factors. Yet for all this complexity of the subjective aspect of the learning, the actual attempts at systematic reasoning were crude in the extreme, as measured by the simplicity of the problems when presented visually. It was thought not only desirable but necessary to conduct an experiment especially designed to emphasize the ability of the subject to think coherently and systematically, or to reason. Accordingly, Experiment III was conducted with this object in mind.

In order to control this special phase of the learner's activity for the purposes of a more exact investigation, two conditions were observed in the experiment. (I) The subject was instructed to take an overtly rational, thinking, reasoning attitude towards his learning, even if he became convinced that such a procedure did not count towards the greatest efficiency. In the

preceding tests, it will be remembered, he was permitted to take any attitude he saw fit. (II) Two mazes were built, which in addition to offering difficulties similar to those encountered before, presented special formation designed to elicit the maximum amount of reasoning.

1. Apparatus

(*a*) The mazes employed were of the pencil variety. For this experiment, and for future investigation, the writer designed a maze base, upon which any alteration in maze pattern could easily be made. Upon the upper surface of a solid oak base, 14½ by 20 inches in size and 1½ inch thick, two series of parallel grooves were cut, extending from side to side and from end to end, so that they intersected at right angles, and cut the upper surface up into a complete checker-board. The grooves were uniformly ⅛ inch in thickness, ½ inch deep, and ½ inch apart. Into them were inserted steel strips to serve as the sides of the maze paths, and between these strips brass flooring was laid. With an assorted number of brass and steel strips, any desirable combination of path sequences could be easily constructed. For the open space built into maze M, (see diagram, Fig. 12, and description) a solid brass plate, of the same thickness as the flooring, was laid. The exit of this maze was in the interior, at the end of a blind passage. An electric buzzer was used to inform the subject when he had attained it. The brass flooring in this passage was cut off so that it lacked 1½ inches from extending to the end wall, and the floor was continued by a thinner strip of steel, slightly raised above the wood base, and free at its outer end, so that pressure upon it made an electric contact with a wire run through the base of the maze, and started the buzzer. Two mazes, L and M, were constructed with this apparatus, and used in Experiment III.

(*b*) *Object of the designs of the two mazes:* Maze L. The pattern of the path formations in this maze was elaborated to present difficulties by way of the similarity of two different paths. Similarity in the mazes previously used had made discrimination extremely difficult. In maze L it was attempted to arrange the

paths in such a manner that this difficulty could be overcome by a relative reliance upon reasoning processes, as over against a trial and error method. [See Figure 11.]

The object of the experiment in brief was this: Two paths, A and B (in maze L), were of sufficient relative length to set them off as distinct land-marks for the learner, but since they

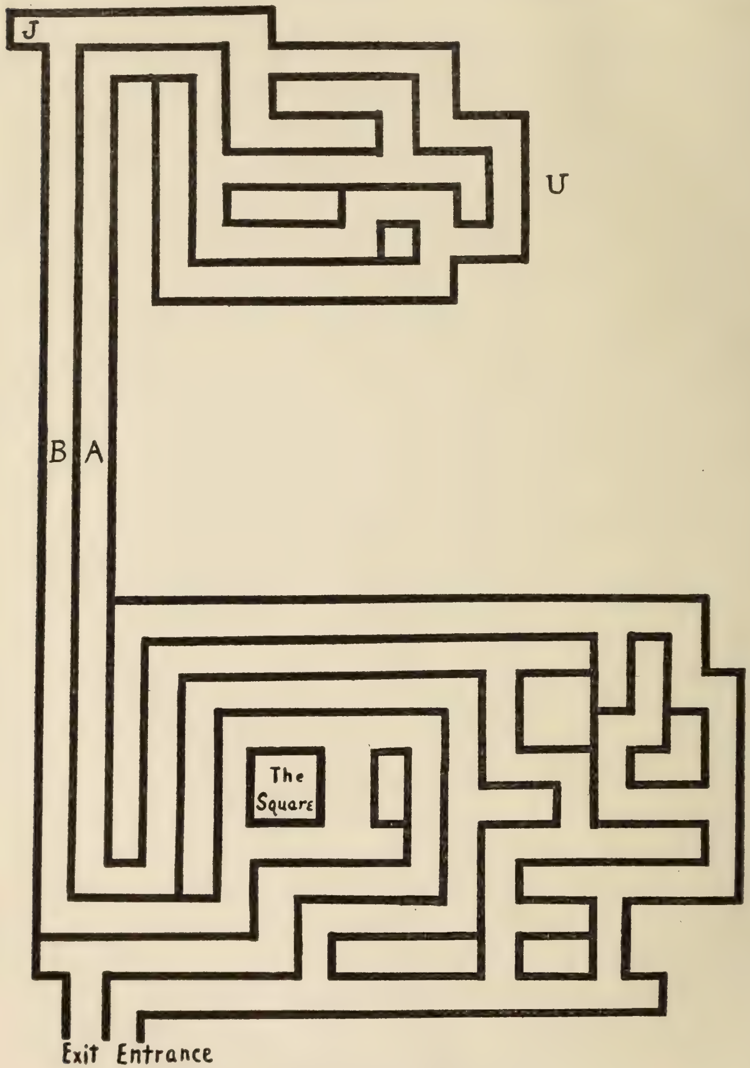

FIGURE 11. Maze L.

were similar in position, and in their respective turns at either end, the difficulty would be in ascertaining whether they were really two paths or one. Efficient learning, however, depended upon the subject's ability to determine this point: if from the lower part of the maze only one long path ascended to a cul-de-sac in the upper part of the maze, then it was useless for the subject to traverse this path, since he had to return by it. If there were two paths, there was the possibility that both were sections of the true path (as was actually the case).

It was obvious that each subject would: (1) raise this issue as just formulated, or (2), would assume without question on the start that there was but one path, only to be disillusioned later, or (3), would perceive on the first trial that there were really two paths. It was correctly assumed by the experimenter that one of the options indicated under (1) or (2) would characterize the learning of the majority of the subjects. As a matter of fact, only two of them saw from the start that there were two paths, and accordingly, no special difficulty presented itself to these two.

The two paths were parallel, and so alike in length, that discrimination on this score was found to be practically impossible. At the top, the three turns and one cul-de-sac leading off from the paths were similar; at the lower end, both turned to the right, then up, then right, with an option on the last turn. One distinguishing mark was made. Path B had at its immediate upper extremity a square opening to the left, while A at its top permitted the subject to turn only to the right.

It was assumed that logically a reasoner, in attempting to discriminate or differentiate between two things in any problem, would seek some distinguishing mark. In this case, that mark was put directly on one of the paths. Logically, the subject could formulate the problem as follows: "If there are really two paths, the possibility is that one of them presents some point of difference from the other. This distinguishing sign is to be sought, first in the path itself, and next, in the respective sequences of turns at either end."

Maze M. In designing this maze [Fig. 12] the experimenter

was prompted primarily by the fact brought out in the "Mouse-trap," where the controlling *idea* that the exit was to the outside of the maze distinctly prolonged the learning of some of the subjects. In maze M the exit was at the place marked $\times$ and the subject was informed when he had reached it by the fact that a buzzer was sounded. He was given none of this information however, at the beginning of the first trial. Once again

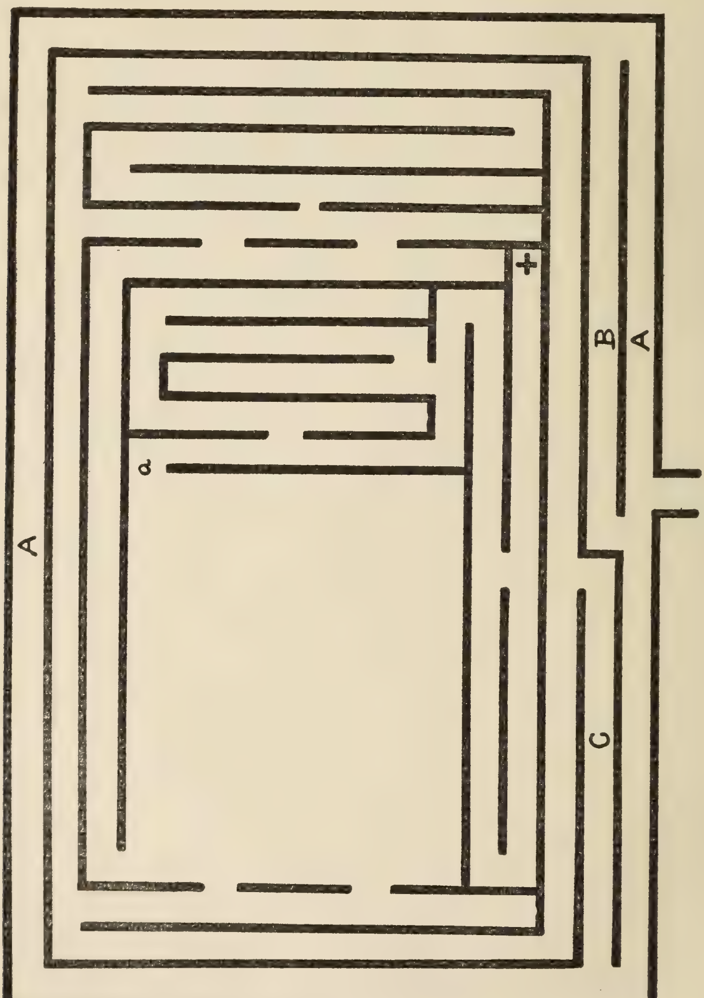

FIGURE 12. Maze M. Exit is marked by cross.

we attempted to give the subject a cue which could be reacted upon in a reasoning way. Path A completely encircled the maze, and brought the subject back to the entrance. Since it offered no other opening to the outside, the exit must be somewhere within the maze.

It was assumed that most of the subjects would make the tour of path A in the first trial. As a matter of fact, all of them did. Since they were asked to use a rational attitude, to keep themselves alert for material to think about, it was thought that such a formation would offer the necessary data.

Two additional points were in mind in designing both mazes: (a) Several cul-de-sacs, like those in the park maze, represented deviations from the typical cul-de-sac formation, such as that found in the first maze we employed. It was desired to study the reaction the subject would make to a new situation, under the new instructions, and to determine whether or not new constructions deterred the learning. (2) The subject was asked to learn the shortest route. As a separate problem, it was desired to ascertain the manner in which the subject could prove that the path he had learned was the shortest.

2. Method

The greatest innovation introduced into the method was the instruction mentioned, that the subject was to do all the thinking and reasoning possible in his learning. Since any given problem often extended over several trials, it was thought best to hold them in succession, with 10-15 minute intervals between trials. The maze was clamped to the table after the manner described in Experiment I. The subject faced the entrance side of each maze, so that U in maze L was called the upper or top side of the maze. The following subjects learned the two mazes: J. R. A., J. W. H., M. H. S. H., M. R. F., S. M. R., C. N., W. S. H., R. B. O.

3. Results

(*a*) *An analysis of the learning method employed in the solution of the different problems presented:* The observation of

the objective behavior gave slight indication only, if any at all, that a different working attitude had been imposed upon the learners. It was practically impossible to tell by the keenest watching what schemes, if any, were being acted upon. The subjects however found that by commenting aloud they were all the more able to formulate their ideas, and in this way, as in the park maze, the observer was given a check on introspective reports and on behavior.

(1) The parallel paths in maze L. The plan of the parallel path arrangement was a source of serious trouble for most of the learners. Maze L proved to be the most difficult maze to learn employed in any of the experimental work. The following summarized accounts give the essential aspects of the methods employed by the learners.

(i) Subject R. B. O. This subject spent two hours and a half, distributed over five trials, in the situation offered by the maze—(the greatest total time spent in learning the first maze, Experiment I, was less than one hour). In trial (I) A was noticed and commented upon as being extremely long when he first ascended it to the U region. Here he became confused, and when he finally found the exit through B, he assumed that there was one path only, leading to a complicated cul-de-sac at its top. No progress was made in trial (II). In (III) he hit upon the idea of two paths, but in an attempt to escape from U he repeatedly came down A, thinking it was the escape path B and he began to doubt his theory. In (IV) the subject went almost directly from A to B through U several times, but conceived the the idea that he was traveling in a circle, and refused to come more than half way down either path. He began to question all the more the possibility of two paths, and by the end of the trial he definitely decided there was but one. Trial (IV) was continued into the next day, as the subject was fatigued at the end of 36 minutes. In this second attempt the subject spent a full 15 minutes trying to find the exit without going up A. He resorted to the scheme of going half way up this path, and then retracing down, because, as he said, "I find the exit after coming *down* this path." Several times he reached the first

turn off A at U but refused to go farther: "I know all about *that* territory, (i.e., knew it was cul-de-sac region) and don't intend to get mixed up in it." He did finally as a last resort explore the region and found himself in J. Then he made the discovery: "There *are* two of them, because the other one doesn't have this notch at the top." The subject had entered J several times before, in the preceding trials, but it had not attracted his attention.

(ii) Subject M. H. S. H. The idea that there were two paths occurred to this subject in (I) but she located the descending path to the right of the ascending one, on the basis of the kinaesthetic feel of arm stretch. By (IV) she was skeptical of her theory of the two paths: "They feel just alike, but sometimes I get out after coming down the long stretch, and sometimes I don't." In (VII) she spent 23 minutes attempting to find the exit without going up A, and on this basis, assumed that after all there were two, and also assumed, without question, that the one to the right was the descending path. By (IX) she was suspicious again: "There really seems to be but one path, and the turns at either end are alike, but sometimes it doesn't lead me to the exit." That is, like the other subjects, she often came down A thinking it was B. In (X) it occurred to her for the first time, in describing the maze in her introspection, that if the ascending path were to the left, the two paths must intersect somewhere in L, since she had found that the exit was to the left of the entrance. In the next trial the subject descended B twice in succession, went around the "Square," and back to U. She was quite sure, however, that she had taken a different descending path each time. Therefore she was certain that A and B intersected in L because they both led into the same region in that vicinity. In answer to a question, she said she could not prove it, or reason it out—there was nothing to reason *on*. But there did not seem to be room at the bottom for two paths so nearly alike, and stretching over so much territory. Therefore there was perhaps only one path after all. In (XIII and XIV) she accidently noticed that a joint in the floor in the lower end of A was more uneven than the one in B. There must be two of

them, therefore. This being settled, trial (XVI) was directed at a more specialized problem: since she had established the identity of the two paths, the question was to tell which she was entering from U—they both felt alike, and she had no way of telling until she reached the bottom. In (XVIII) she discovered J for the first time (she had been repeatedly entering it), found that a similar projection did not mark A, and soon learned to go through this part of the maze without error. She had gradually given up the idea that A was to the right of B.

(iii) Subject M. R. F. The learning of this subject followed very closely that of M. H. S. H. Like her, she raised the question of the possibility of the existence of the two paths in the first trial, and similarly, she thought that the ascending path was to the left of the descending path. This suggested at once however, the two must intersect somewhere at the bottom, since she was sure of the correct relationship of exit and entrance. The subject resolved to perfect the route she had learned before she attempted a study of the situation. By (VI) she had the path fairly well learned, and in this trial also she discovered that J, which she had noticed before, was at the top of B only. This landmark she retained as a cue to tell her on which path she was descending. The subject finally decided that she had misjudged the position of the two paths.

(iv) Subject J. R. A. This subject in his first trial ascended and descended A several times before finding B. He immediately assumed, at the close of the trial, that the two paths existed, because one got him out and the other did not. He also correctly assumed that the return path was the outer one, because it seemed to extend farther up than path A. The rest of the learning presented no serious difficulty to the subject.

(v) Subject C. N. This learner believed after the first trial that one long path ascended to a cul-de-sac territory at the top of the maze, and that it was not necessary to go up this path at all. Most of trial (II) was spent in an effort to find a shorter cut to the exit, but after 20 minutes endeavor in this direction, she decided to try the possibility of the long path. In doing so she discovered J, remembered that she had found it in coming up, and concluded that there were two paths.

Of the other two subjects, the reports of J. W. H. follow those of J. R. A., and the account of W. S. H. is similar to that of the subject just described.

(2) The location of the exit of maze M. The discovery of the inner exit of this maze was practically a matter of accident, as it was in the case of the park maze. M. R. F. in her first trial directly made the circuit of the outside path A several times, in one of which she kept consistently to the outside wall. Although she recognized the nature of the path, as leading her back to the entrance, and although she had the idea rather definitely in mind of finding the exit somewhere on the outside of the maze, the fact that it could not be there did not suggest itself. Her motive, when she did finally approach the interior, was to get *off* this path. She reports however a vague and non-focal state of questioning *how* the surrounding path would affect the rest of the maze.

J. W. H. reports that he did wonder vaguely about the exit after he had made the circuit of A. He did not formulate this into a definite question however. He had conceived of the electric wiring as a device to mark cul-de-sacs, and did not connect it with the exit.

J. R. A. stated that his definite working scheme of making all the right turns tended to exclude questionings about the exit. He too went around A, sticking closely to the outside, without perceiving that the exit must be within. This report is an exact duplicate of the testimony of the same subject in the "Mouse-trap."

No subject got the suggestion of the exit after encircling the maze, although all of them did so in the first trial before they located it.

(3) The perception of new formations in the mazes. Conclusive evidence had been offered in the park introspections in the case of R. B. O. with x and y, and with the other subjects in regard to the exit, to indicate that a novel arrangement of paths or cul-de-sacs, not only taxed the ability of the subject to interpret new situations, but was directly the cause of a delay in the learning. Each subject applied some concept to the new

kinaesthetic and cutaneous experiences: the one that gave the most immediate and satisfactory meaning to the experiences was accepted. Such conceptions proved to be inadequate quite often, from the point of view of practical results.

Subject S. M. R. in maze L offered an instance of such an inadequate interpretation, in her idea of the "Square." [Fig. 13.] For a full period of 20 minutes in trial I she refused to come down path B, because of the fact that the first time she had done so she went directly around the square and up B, and conceived the situation to be as she represented in her drawing. As was the case with so many conceptions concerning the maze, it was a matter of assuming a situation without question, rather than a process of formulating an option, or criticizing the conception. An example of this was brought out in Experiment I, in which the subjects so many times went to a turn in the true path, and retraced, thinking it was the blind ending of a cul-de-sac.

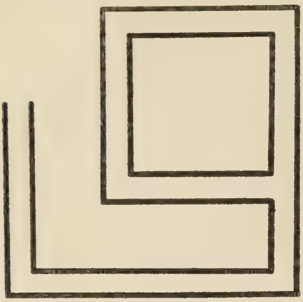

FIGURE 13. The "Square" of S. M. R.

About half the subjects in their first experiences with the "Square" interpreted it as did S. M. R., but they discovered their mistake before the idea resulted in serious harm.

The record of this same subject presents another instance of uncritical judgment of maze formations in maze M. Early in trial (1) she got into the vicinity of the exit, and conceived the whole area immediately to be cul-de-sac territory. She refused for a long time to enter door *a* because, having located it as the entrance to that region, she said: "Anybody ought to have sense enough to avoid a place like that." This conception, after she had explored thoroughly the possibilities of the surrounding path A, delayed her reaching the exit for a full half hour.

Several subjects in maze M started to the right and came back on B to regain A, in the same way that W. H. S. made 11-12 a

part of the true route in the "Mouse-trap." They accidently discovered sooner or later the nature of the paths.

The cul-de-sacs to the immediate right of the open area in maze M proved to be the most difficult situation to formulate in the maze. The open space itself was peculiar without being complex, and the one thing that made the path easy to learn.

The general result may be summed up in the statement that new situations retarded the learning, unless they were so simple and distinct that they afforded definite cues for orientation, in which cases they facilitated the learning.

(4) The shortest path. The discovery of the shortest path proved to be a matter of difficulty to only two of the subjects. The others, after having once learned to turn left on C, easily learned to enter the open space through the upper left hand area and go diagonally across it. The special possibilities as to relative lengths in general were perceived, as a matter of sensory discrimination, without effort.

M. R. F., however, learned the route as turning to the right on C, then up, and across the top of the maze to the left on the corresponding path. Her discovery of the left side route was purely accidental according to her reports, and was the result of going too far to the left in one trial as she was making this trip, and coming back to the entrance. W. S. H. did not learn the shortest path from the open area to the exit until after he had habitually turned up after leaving it, and had come down the path directly over door *a*. His discovery was accidental, and similar to that of M. R. F.

(5) The nature of the attitude employed in Experiment III. The subjects were unanimous in their statements that the method employed, in spite of heroic attempts to follow the instructions given, was not materially different from that employed in the other mazes. The nature of the type of reasoning they found it possible to do was practically the same as that indulged in when they were left to their own method. The common statement was that they had nothing to reason *on*. The data had to be acquired by trial and error, and the significant discoveries were made in such hit or miss performances.

They did assume an attitude, somewhat forced and artificial, differing slightly from their natural method of attack. This was found to consist in (1) an attempt to 'think-out' certain possibilities by way of theory or conjecture, or to follow a systematic method of exploration. Thus W. S. H., maze L (I), speculated as to which side of the maze the exit might be located. Thus J. R. A. and others followed one side of the path, exploring all its possibilities first. (2) A second characteristic of the process was the more acute attention paid to experiences and cues. This attention was concerned at times with a definite object: several of the subjects, after they had formulated the difficulty in maze L, were overtly on the lookout for distinguishing cues. What they did not do was to search for these cues in a systematic manner. This was practically an impossible task, so they reported, since the major part of their attention had to concern itself with immediate orientation, and since also they were forced to stay in the maze paths—had they been permitted to jump from one section to another, more systematic exploration might have been possible. The two distinctions given were relative only: it was a process of slightly more emphasis upon methods that all of the subjects employed, and which they could not help employing.

(*b*) *The objective results:* The sudden decrease in the time and errors in the 2-5 trials indicate without exception, in maze L, the period in which the nature of the two paths had been discovered. The extended continuation of errors resulted from the fact that the rest of the maze still offered a series of minor problems, in the shape of small cul-de-sacs, easy to escape from but difficult to avoid. The explanation given in the account of the objective records in Experiment III applies without exception to the records from this maze. The same type of abrupt decrease also characterized the results from maze M, and the fact that the time and error curve based on it is slightly more irregular finds sufficient explanation in the fact that the maze itself was more complex. The stage in which the subject learned to avoid the entire right part of the maze is marked by the fall of the curves. The open area in this maze undoubtedly saved it from being more difficult than maze L, the most difficult one we designed.

Subject	J. R. A.		C. N.		M. H. S. H		M. R. F.		W. S. H.		J. W. H.		S. M. R.		R. B. O.	
No. of trial	Time	Errors	T	E	T	E	T	E	T	E	T	E	T	E	T	E
1	10′ 57″	148	54′ 40″	427	16′ 45″	188	5′ 10″	33	38′ 40″	447	8′ 30″	103	1 hr. 12′ 24″	565	39′ 40″	435
2	9 56	130	20 20	194	6 52	67	2 37	38	13 10	295	7 38	98	5 50	30	6 45	184
3	7 16	67	3 37	50	3 24	25	2 54	20	12 10	150	5 10	75	6 45	72	48 45	695
4	8 05	113	44	5	5 32	49	2 46	20	1 13	12	2 30	35	2 51	30	36 37	600
5	30	4	47	0	1 00	9	1 09	9	2 20	24	16	2	2 06	4	18 36	175
6	29	0	34	3	59	3	1 35	10	36	10	14 37	198	1 10	4	5 75	75
7	30	1	40	0	4 33	23	3 07	29	27	1	1 13	9	1 26	7	3 07	62
8	35	8	30	2	2 07	27	2 04	7	22	0	22	2	59	2	4 37	59
9	20	1	30	2	2 47	18	2 33	16	27	3	41	1	44	2	2 57	13
10	47	9	40	0	1 27	16	1 32	9	37	1	34	2	45	1	2 32	20
11	20	0	34	0	3 28	34	1 25	20	30	2	39	0	46	0	2 51	2
12	18	1	32	2	5 16	83	47	5	42	2	34	0	1 20	13	44	13
13	1 14	30	27	2	4 48	40	40	5	36	0	30	0	40	1	26	4
14	20	1	33	0	5 56	64	58	17	47	5			55	6	29	1
15	23	1	35	1	5 26	92	35	0	37	1			40	0	24	0
16	37	12	27	0	3 14	39	38	3	24	0			40	1	26	0
17	26	0	35	2	9 45	118	37	2	21	0			42	10	29	3
18	25	0	40	2	1 27	2	30	4	24	0			30	2	29	0
19	24	0	37	8	1 04	0	27	3	7 14	110			42	2	30	0
20	3 50	70	30	0	1 07	1	22	1					32	1	30	4
21			25	2	56	0	26	1					44	0	19	0
22			40	2	50	0	26	6					39	2	17	0
23			37	0	50	0	21	1					44	8	28	10
24			25	0	44	0	22	2					47	6	22	5
25			27	0	1 23	14	42	8					32	0	20	5
26							24	0					47	7	25	3
27							22	22					35	0	25	3
28							24	1					30	0	25	5
29							21	1					30	0	25	4
30							18	0					25	0	22	0
31							17	0					1 16	15	32	13
32							3 50	30							(Learned at Trial 57)	
Total	47′ 32″	636	1 hr. 37′21″	809	1 hr. 30′59″	912	41′ 40″	317	1 hr. 9′33″	1039	43′ 48″	525	1 hr. 49′ 6″	791	4 hr. 34′ 35″	2455
Average	2′ 22.6″	31.8	3′44.6″	31.1	3′47.4″	38	1′ 18.1″	9.0	4′11.2″	5.46	3′ 22.2″	40.4	3′ 32.4″	26.4	4′1.1″	56.8

TABLE 1.—Time and Error record, Experiment III, Maze L. Results from each trial, up to and including Trial 32, are given. Totals and averages are computed from the complete records.

Subject	J. R. A.		C. N.		M. H. S. H.		M. R. F.		W. S. H.		J. W. H.		S. M. R.		R. B. O.	
No. of trial	T	E	T	E	T	E	T	E	T	E	T	E	T	E	T	E
1	3′ 26″	36	11′ 45″	73	7′ 37″	47	7′ 12″	58	8′ 11″	35	21′ 35″	147	1 hr. 4′ 49″	490	11′ 5″	98
2	1 38	16	2 35	28	2 33	34	1 00	9	2 14	38	50	3	1 10	5	33	0
3	41	10	49	10	1 57	15	1 02	8	1 55	14	31	0	2 47	28	27	4
4	1 38	18	1 31	14	50	17	45	5	5 21	56	13	0	50	10	27	4
5	2 35	28	2 42	27	50	7	45	5	5 21	56	13	0	52	5	52	8
6	16 40	60	1	20	1 05	8	25	6	3 07	23	9	0	37	5	22	4
7	48	5	58	8	50	5	25	6	3 38	30	10	0	30	3	15	1
8	1 15	9	50	10	51	5	25	5	3 12	39			29	3	16	1
9	3 42	30	45	6	34	4	17	4	5 58	63			30	5	15	0
10	29	2	52	6	35	4	20	4	59	6			24	3	13	0
11	33	2	1 24	13	12	15	15	4	48	6			20	4	12	0
12	17	0	33	6	2 19	23	17	4	34	6			21	4		
13	15	0	33	6	47	8	18	5	50	4			1 20	20		
14	15	0	30	6	1 12	8	1 00	11	50	4			39	12		
15			1 50	18	42	4	15	3	1 48	14			34	4		
16			30	2	1 04	9	15	3	2 13	17			25	6		
17			47	3	29	4	27	5	1 32	8			1 00	7		
18			2 14	27	30	4	29	6	30	0			35	7		
19			1 10	10	1 17	19	18	3	6 03	87			27	2		
20			27	2	27	4	17	3	37	0			31	2		
21			27	2	27	4	14	3	31	0			42	10		
22			30	2	25	4	2 00	19	27	0			20	0		
23			53	8	1 00	10	3 29	20					44	12		
24			40	9	1 36	20	22	0					24	2		
25			29	2	27	2	20	0					18	1		
26			24	2	52	10	15	0					12	2		
27			25	2	2 37	26							34	7		
28			42	2	1 47	26							50	7		
29			30	2	2 05	20							17	0		
30			58	3	36	2							15	0		
31			55	5	47	3							15	0		
32			1 15	12	47	3										
33			45	5	39	3										
34			57	7	53	5										
35			43	3	32	0										
36			(Learned at		3 13	0										
37			Trial 47)		20 12	0										
Total	34′ 12″	216	44′ 38″	351	1 hr. 5′50″	376	23′ 7″	199	52′ 35″	426	25′ 8″	160	hr. 24′ 1″	666	14′30″	116
Average	6′26.6″	16.6	1′16.5″	10.3	1′49.7″	10.4	53′ 3″	7.7	2′ 23.4″	21	3′ 35.4″	22.9	2′ 42.6″	21.5	1′ 27″	11.6

TABLE 2.—Time and Error record, Experiment III, Maze M. Results from each trial up to Trial 37 are given. Totals and averages computed from complete records.

The last trial for several subjects, after the maze had been run three times without error, shows a sudden increase in time and errors. This was occasioned by the fact that many of them chose to perfect their route first, and then explore for a possible path, in compliance with the criterion established for learning. The last trials in such cases represent the time and exploration necessary to satisfy the subject that no shorter path existed.

E. EXPERIMENT IV

The reports from the preceding experiments were unambiguous to the effect that the different attitudes attempted towards learning did not involve actual differences in method. The assuming of a reasoning point of view carried with it a more evident play of ideas, but these ideas as manipulated by the learner did not directly solve maze problems. The results suggested the possibility that the conditions of the learning task made only one method possible, or at the most permitted deviations from this method. The evidence on this point was not conclusive, however, and an experiment was designed to bring out the variations possible.

1. Apparatus and Method

(*a*) Three similar pencil mazes were used in Experiment IV, one of which is shown in the diagram, Figure 14. The paths, ¼ inch wide, were cut through brass plates, 8 inches by 10 inches in size, and 3/16 inch thick. The mazes could thus be turned over and be used mirror fashion. The exit and entrance ends of the paths did not lead out of the mazes, but terminated blindly, like cul-de-sacs. To mark the exit, a brass wedge, fitting closely into the blind end of the path, acted as an inclined plane, and carried the pencil out of the maze, so that the subject knew immediately when he had completed the trial. The wedge could be placed at the end of any blind passage, and thus the exit as well as the entrance of the maze was variable. A wood base, covered by a plate of glass ¼ inch greater in length and breadth than the maze, was used as its support. Four strips of brass, of the same thickness as the maze, formed the border of the top of the base. They enclosed a rectangle slightly larger than

the maze itself, and thus allowed it to fit tightly upon the glass, upon which a sheet of paper could be placed for the purpose of preserving a graph. In addition to holding the glass, the brass border kept the maze firmly in place. The apparatus was easily clamped to the table.

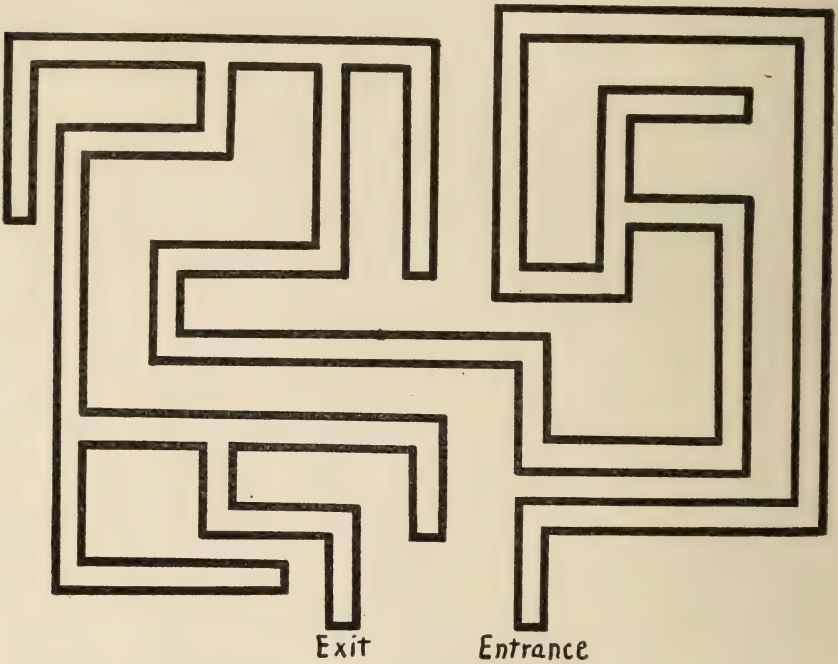

FIGURE 14. Maze I b.

(*b*) *Method:* The subject was asked to learn three mazes, in successive trials, each maze at a different sitting. He was instructed to take toward each a separate and characteristic attitude, which was defined and explained as follows:

(1) His "Natural" attitude. The directions were identical with those given in Experiments I and II.

(2) An attitude of "Conscious Trial and Error." The learner was asked to make the affair as conscious as possible, to attend to, discriminate, and remember paths, but not to reason, or speculate, or indulge in ideational activity not directly concerned with the motor processes to be employed.

(3) An attitude and working method of "Surplus Activity." He was instructed to move as rapidly in the maze as he was able. He was to make no *special* conscious effort, save that necessarily involved in maintaining the speed called for. Since the distraction tests mentioned in Experiment I had convinced us that complete distraction of the attention was impossible, the subject was not asked to concentrate upon some extraneous topic—that is, he was allowed to employ as much consciousness in the learning as the actual speed conditions permitted.

The point was emphasized that a premium was placed upon efficient learning only in as far as it was accomplished in terms of the directions given. Several subjects had stated in Experiment III that the rational attitude was not conducive to the best learning, and it was made plain that objective results were to be sacrificed for the attitude desired.

In designing the mazes, it was attempted to equate them in the matter of complexity. In order, however, to eliminate the factor of the influence of the individual maze pattern, different mazes were offered to each subject for each of the three methods. The mirror form was employed in two cases, but it was arranged that no one subject was given both the maze and its mirror form to learn. At the top of the columns in the tabulative statement [page 98] is found the number indicating the maze used.

The subjects in this experiment were J. R. A., J. W. H., M. H. S. H., M. R. F., C. N., W. S. H., and R. B. O.

2. Results

(*a*) *Reports of the different subjects on method:* The subjects were asked at the close of the experiment to write a detailed introspective analysis, based on a comparison of different methods used in all the mazes they had learned, in this and in the preceding experiments. The essential points of these analyses, expressed as far as possible in the subjects' own terms, are as follows:

(1) Subject M. R. F. The method employed in the "surplus activity" learning was somewhat different from that used with the other mazes. This was the result of the unnatural speed re-

quested in the performance, which interfered with the fixing of associations, partly by the rapid shifting of attention, and partly by the distracting effects of the disagreeable affective state, and the actual strain and fatigue involved. Because of this, segments of the maze were learned as a hand-motor coördination, without conscious control.

These segments, however, were those in which little opportunity for error was present. In times of difficulty such a method was inadequate, and the crucial turns were figured out by essentially the same method used in all the other mazes. The solution was delayed, however, by the conditions imposed.

The attitude taken in the other mazes presented little variation in learning method. Where reason was requested, cues were followed up a little more consciously, and in the intervals between trials, the problems were studied a little more.

On the whole, there was sufficient similarity between the different methods to warrant their being considered as phases of one learning activity, but the "surplus" method represented the most pronounced divergence from the regular procedure.

(2) Subject J. W. H. There are two methods possible by which a maze may be learned, but they are different only in the relative emphasis placed upon common factors. The first was represented in the "speed" maze, the second in all the others. Both involved discrimination and memory, but in the second instance this is more deliberate, more overt. It involves a more conscious attention on cues, and often conscious exploration to find these cues. More emphasis is placed upon the constructing and following out of a visual diagram. In the "surplus activity" maze, little emphasis was laid on the visual element—motor imagery was practically the only type used. J. W. H. was able to describe the true paths in all the mazes when learned, but he knew less about the cul-de-sacs in the maze learned by rapid movements than in the other mazes, although he was out of the true path a relatively longer time. This he ascribed to the fact that chance success through surplus activity characterized the learning in this maze, rather than chance success by means of deliberate exploration.

(3) Subject M. H. S. H. This subject finds only one method available in learning mazes. She attempted in the "speed" maze to learn it in motor terms—i.e., by letting each successful trial help establish a habit. She assumed that the true path would be traversed oftener than cul-de-sac paths, and that they would as a consequence be finally eliminated. But her experience proved that this method resulted in false paths becoming incorporated into the true, hence conscious avoidance of cul-de-sac entrances was essential for every maze she learned. The learning by surplus activity approximated unconscious learning more than the other tests, but the methods employed were essentially the same for all the mazes.

(4) Subject W. S. H. also reported that only one method was possible in the actual process of learning to run through a maze without error. The process is one of discriminating and remembering crucial points. Normally, every turn and path is attended to at the start, whether it presents chance for error or not, and those found free from dangerous situations are dropped out from consciousness. In the "surplus activity" learning, he attended only to those crucial regions, as a series of definite problems, when they forced themselves upon his attention. Therefore there was more surplus ideational activity in the rational and natural methods—surplus in the sense that all of it was not absolutely essential to the act of learning.

(5) Subject C. N. This subject made a two-fold distinction of methods on the basis of the voluntary attention paid to cues and suggested ideas when she made the learning a studied one, as over against the type of process represented in the "surplus activity" method, in which, through repetition of errors, the decisive terms were forced upon her consciousness. Her analysis follows rather closely the description given by J. W. H. and W. S. H., since it places rational learning as one extreme method, as opposed to learning by the method of rapid movement. She drew the distinction, however, in terms of the volitional effort attendant upon the one, as contrasted with the other in which the control ideas were forced upon her.

(6) Subject R. B. O. The essential distinction that this sub-

ject's introspection gave him was that apparent to W. S. H. In any rational method of solving maze problems, the attention at first concerns itself with every experience and every suggested cue, although some of them are found afterwards to be non-essential. In the non-rational method, these experiences are not attended to until either a chance success forces some of them upon the consciousness of the learner, or repeated entrance into a cul-de-sac does the same. In such instances, attention to them is necessary and involuntary, and is indispensable for learning.

(*b*) *Summary of the analysis: the nature of the method:* The similarity of the different reports leaves no doubt that to the minds of the subjects the learning under different conditions presents simply phases of the some process. One aspect involves more than the other, of what was variously defined as consciousness, volition, effort.

The different mazes called forth these distinct attitudes only partially. In the original pencil maze, and in the "Mouse-trap," there was a constant, almost periodic, fluctuation between these two extremes. Sometimes helpful cues were obtained with one attitude, sometime with the other. The fluctuations were due to fatigue, or to a shifting opinion as to the values of the two methods, or to the type of local situation in which the subject found himself. The testimony was unambiguous to the effect that in crucial points the *idea* of which way to turn, or not to turn, was necessary. At times the subjects were definitely on the lookout for such ideas, at other times the essential controlling information was suggested to them involuntarily.

Of the two extremes, the one imposed by Experiment III, the other by the "surplus" method, the subjects were in accord in the statement that they were both artificial and futile. Neither was a "natural" method. They could not learn a maze by "reason," neither could they by a technique tending to eliminate the type of consciousness involved in the process—voluntary attention, discrimination, judgment, suggested working ideas, memory. The "natural" method, and the method of "conscious trial and error" were found to be identical, and the latter phrase was accepted as a just characterization of the processes involved, if it

were taken to include something more than a mere passive retention of cues discovered accidentally.

Numerous writers have of late formulated a modification of the conventional antithesis between "trial and error" and "ideational" learning. Ruger[4] mentions the fact that the hit-or-miss method was in evidence with his subjects in their attempts at the solution of puzzles. Colvin, who cites Ruger's results, refers to the trial and error method with human learners, but emphasizes the fact that the learning endeavors made by human beings are not aimless, but are controlled by anticipation of probable results: "When, however, we have reached a higher stage of development, particularly in man, we may assume, as we have already pointed out, that trial and error is something more than a hit-or-miss process through which an adjustment is finally secured. The trial becomes a conscious one, and is self-directed." [5]

Correlating the results of this with the previous experiments, we conclude that, in general, the method of learning was conditioned by the nature of the maze, rather than by the attitude of the learner. In proportion to the extent that the maze was complex, a greater amount of conceptual control and reasoning were called into play. In any simple maze, the method approximated a process of mere trial and error. A certain amount of discriminating and memorizing was necessary, and little opportunity was given for the play of higher processes. *The learner has control over the method to this extent: whether the sum total of these activities is to be represented in one trial, or is to be distributed over a series of trials, is largely a matter of choice.* The indications are that the most efficient distribution is a matter of individual variation. The subject tended to set the pace in the first trial that was to characterize his ensuing trials, and he termed this his "natural" method of learning. The speed demanded in the surplus activity test was evidently too great. The subject was not given time for making or fixing associations. No experi-

[4] Ruger, "The Psychology of Efficiency," Archives of Psychology, June, 1910.

[5] Colvin, "The Learning Process," 1912, p. 23.

ment was attempted to determine the speed below which efficiency would suffer.

In difficult mazes, like the "Mouse-trap," simple memory and discrimination did not suffice to give the variability of behavior necessary in order to discover and retain the location of the exit, and the sequence of paths leading to it. Then, more general and conceptual ideas were employed: their function was to reduce to a minimum the number of trials, or to secure *effective* variability. Thus, had the general scheme of the maze just mentioned been given to the subject before he entered it, the bad records for the first trial would undoubtedly have been cut down. The problem was therefore largely one of ideational learning, and the explanation of the divergencies in the objective results is to be found partly in the fact that different conceptions were formed, of varying degrees of correctness, and that all sorts of predilections entered into the process.

F. THE LEARNING CURVES

In plotting the curves, the percentage method of the elimination of surplus values was adopted. This scheme was used by Carr and Hicks[6] in their paper. It seeks to measure in terms of percentage the rate at which excess or surplus time and errors are reduced to zero. The time not absolutely essential for the traversing of the maze is considered as surplus, and it is found by subtracting from each trial, the shortest time made in any trial. As might be assumed, the shortest time made is usually found in a trial without errors; this is, however, not necessarily the last trial. Since all errors are surplus no similar subtraction is made. After the surplus time has been computed for each trial, the time and error records for the first trial are each given the value of 100 per cent, and with this as a basis, the percentage value of each of the ensuing trials is computed. The curve is then plotted upon the percentage results.

The method has two advantages. (1) It brings the time and error records down to the same base line, in any trial in

[6] *Cf.* Carr and Hicks, Human Reactions in a Maze: Jour. of Animal Behavior, Vol. II, pp. 98-125.

	Subject	J. R. A.			J. W. H.			M. H. S. H.			M. R. F.			C. N.			W. S. H.			M. R. F.		
Trial No.	Maze	IIa	IIIa	Ia	Ib	IIb	IIIb	IIb	Ia	IIIb	Ia	IIIa	IIb	IIb	Ib	IIIa	Ia	IIa	Ib	IIIb	Ia	IIb
	Method	Nat.	T. & E.	Surp.	Nat.	T. & E.	Surp.	Nat.	T. & E.	Surp.	Nat.	T. & E.	Surp.	Nat.	T. & E.	Surp.	Nat.	T. & E.	Surp.	Nat.	T. & E.	Surp.
1		1'-17"	5'-12"	1'-30"	3'-45"	2'-06"	1'-17"	1'-24"	-58"	3'24"	-22"	-42"	-22"	5'-14"	2'-28"	-18"	-22"	4'-00"	1'-40"	-55"	1'-20	-39"
2		2 -27	1 -00	-20	-45	-23	1 -01	3 -38	1 -04	-09	1 -50	1 -08	-35	-18	1 -32	-42	-35	-16	-15	2 -40	-21	-23
3		2 -45	-39	-59	-07	-26	-18	2 -48	-18	-36	1 -40	-20	-29	-27	1 -32	-42	-36	-47	-15	-43	2 -27	-24
4		-45	-38	-21	-10	-22	-09	2 -55	-31	-35	-29	-16	-14	-28	-17	-08	-15	-17	-18	-51	-15	-11
5		-33	-15	1 -03	-07	-20	-09	-45	-20	-51	-17	-15	1 -01	-20	1 -00	-09	-15	-18	-08	-25	-18	-23
6		-36	-15	-11	-06		-42	1 -02	-25	-14	-18	-20	-39	-28	[illegible]	-09	-14	1 -57	-13	-20	-23	-11
7		-35	-19	-11			-15	-47	-55	-24	-20	-15	1 -01	-20	-38	-08	-24	-15	-06	-23	-24	-07
8		-33	-26	-29			-26	-25	1 -30	1 -05	-17	-49	-15	-18	-38	-07	-22	-16	-16	-20	-12	-10
9		1 -00	-23	-10			-10	-32	-34	-10	-13	-16	-14	-16	1 -01	-11	-23	-16	-09	-25	-13	-10
10		-41	-20	-09			4 -14	1 -04	1 -04	-09	-13	-14	-24	-17	-20	-12	-13	-11	-08	-20	-14	-09
11		-14	-14	-19			-16	-48	-45	-13	-12	-25	-12		-16	-12	-10	-11	-09	-16	-10	-09
12		-46	-17	-10			-15	-30	-31	-09		-15	-20		-19	-05	-10	-08	-14	-16	-10	-07
13		-21	-18	-15			-20	-21	-28	-47		-13	-11		-24	-05	-09	-09	-09	-13	-13	-07
14		-17	-21	-16			-15	-51	-52	-15		-13	-14		-20	-06		-09	-09	-13	-10	-08
15		-18	-20	-13			-07	-15	-28	-06			-06		-19	-06			-09	-13	-10	-10
20		-19	-20	-10			-16	-20		-38			-08		2 -01	-06			-08			-05
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45			-20	-14						-24			-15			-08			-06			
50			-16	-09						-50			-08			-16						
55				-09						-26						-08						
60				-13						1 -01						-17						
65				-20						-15						-09						
70				-09						-08					(Trial 69)	-08						
			(Trial 94)	-09					(Trial 126)	-12												
	Total	16' 32"	21' 14"	24' 43"	4' 50"	3' 37"	11' 45"	19' 49"	12' 24"	50' 33"	6' 11"	5' 52"	11' 09"	8' 25"	9' 10"	9' 00"	4' 08"	9' 10"	10' 38"	8' 30"	7' 46"	4' 11"
	Average	47.7"	22.1"	15.7"	48.3"	43.4"	21.5"	59.4"	41.3"	24.0"	33.7"	25.1"	13.3"	50.5"	41.4"	9.1"	19.0"	38.5"	11.2"	34.0"	24.5"	11.9"

TABLE 3.—Time record, Experiment IV, showing results for the first fifteen trials, and for every fifth succeeding trial up to No. 70. The number of the last trial is indicated in parentheses in cases in which it does not fall upon a trial number given in the column. Totals and averages are computed from the complete records.

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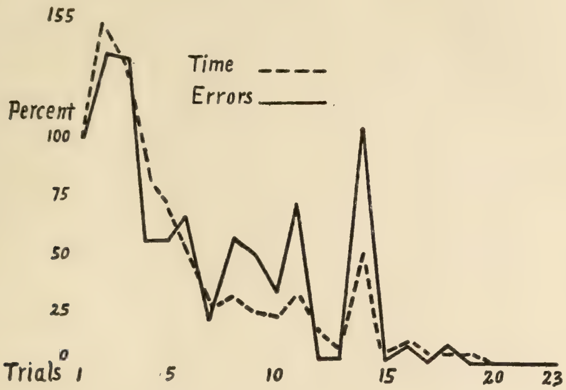

FIGURE 15. Percentage Curve of J. W. H., Normal Maze.

perfect trials represented in the graph. (2) The relation between the time and error curves, i.e., the varying number of errors per unit of time, is readily estimated. For purposes of comparison, two curves of J. W. H. are reproduced, one based on the percentage method, and the other upon absolute time and errors. (Figures 15 and 16.) One disadvantage of the percentage curve is that it is likely to be misleading when the graphs of two or more learners are compared. The records of J. R. A. and M. R. F. (Figures 23 and 24) would seem to suggest that the latter subject learned the "Mouse-trap" with the greater ease. Her time and error results for the first trial were 1 hour, 20′, 56″. and 175, as over against 21′, 37″, and 46 errors for J. R. A. But since the results of the first trial are given the same value for each learner, 100 per cent, the graphs do not afford a ready basis for a comparison of absolute results of the different subjects.

The curves based on the "Mouse-trap" and the Normal maze records were selected for reproduction since they were the two mazes learned without restrictions being imposed upon the learner. The tabular results of the other two experiments however readily suggest what would be the general nature of their curves, if they were actually plotted. Two features of the curves are es-

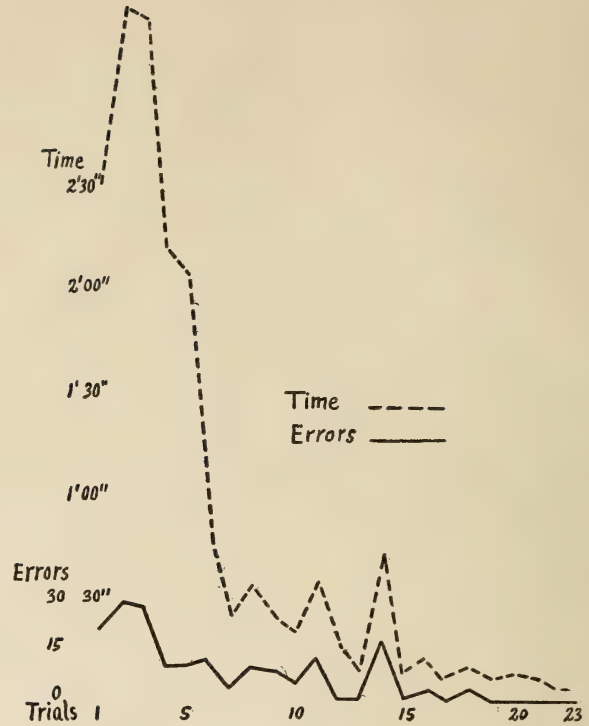

FIGURE 16. Curve of J. W. H., based upon absolute time and error records, Normal Maze. A comparison of this curve with Figure 16 will show the points of similarity and difference in the curves.

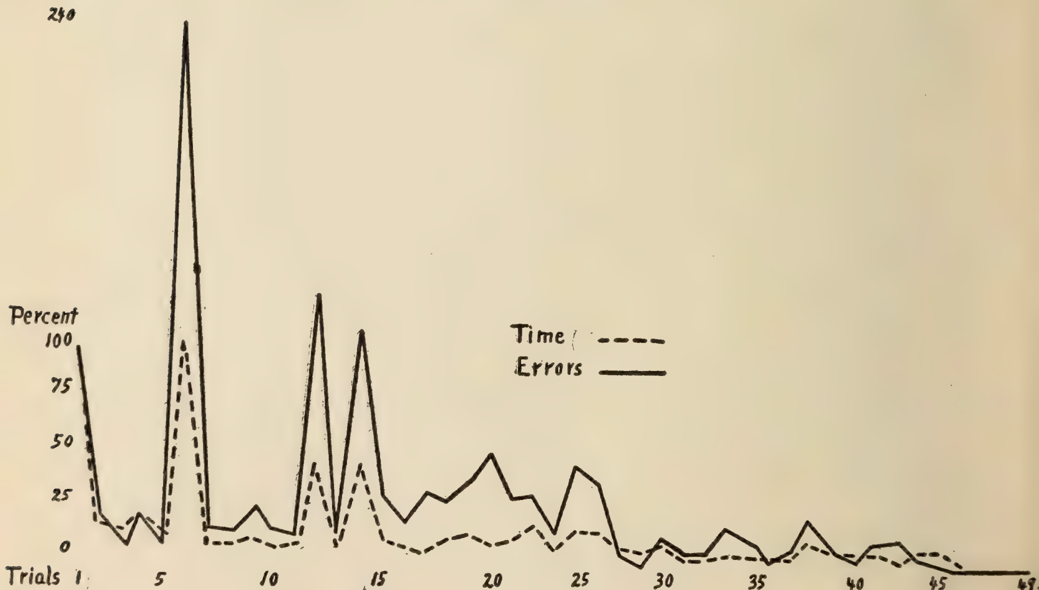

FIGURE 17. Percentage Curve, M. H. S. H. Normal Maze.

pecially prominent: (1) the rapid initial descent; (2) the presence of a marked series of "steeples." The first especially characterizes the "Mouse-trap" curves; the steeples are more in evidence in the curves from the Normal maze.

(1) The conclusion has been urged in a preceding section of the paper that, in as far as there is variation in the learning method, the variation is conditioned largely by the pattern of the maze itself. The results indicate that, on the objective side, there is a corresponding difference in curve characteristics.

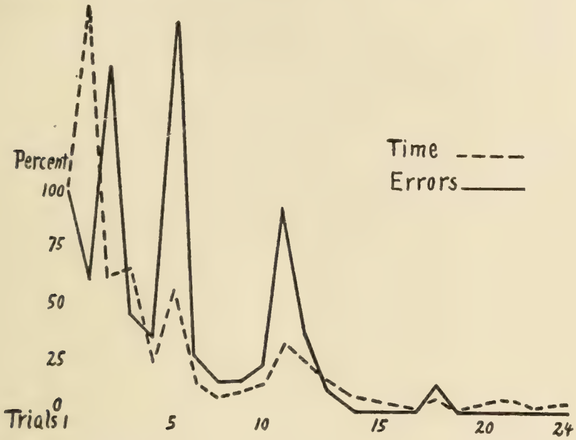

FIGURE 18. Percentage Curve, G. M. F. Normal Maze.

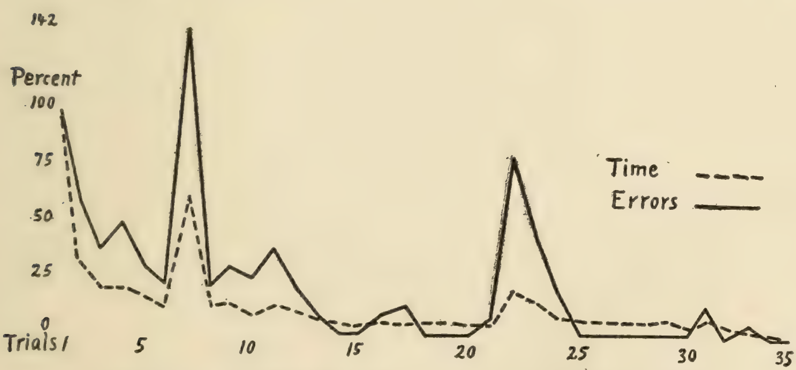

FIGURE 19. Percentage Curve, M. R. F. Normal Maze.

A maze, irrespective of the variable factor of difficulty, may be *complex* in two radically different ways. The path may include a relatively large number of turns, and the individual cul-

de-sacs may be simple, consisting at the most of two or three segments—as, for instance, the "L" form of cul-de-sac. This maze would be difficult in proportion to the number of such blind passages. As opposed to this type, the true path of a maze may be simple, and the number of cul-de-sacs few, but if they happen to be in themselves complex, the situation that is presented to the learner is essentially different from that offered in the kind of maze just described. Of the mazes employed in our investigation, those used in Experiment IV, and sections of some of the other mazes fall under the first class; while the "Mouse-trap," Maze M, the parallel paths in L, and cul-de-sac 6-9 in the first maze used, represent formations of the second class.

Our results indicate that a maze curve tends to show the rapid initial fall in proportion to the extent that the maze involves a short true path with a few very intricate cul-de-sacs. The true path itself is easily remembered when once learned; hence, assuming, as is actually the case, that it is learned in the first trial, only a few trials are required in order to perfect it. Obviously, other things being equal, the general slope of a curve is the more pronounced as the number of trials is cut down. But a complex cul-de-sac either taxes the powers of discrimination to the utmost, or proves to be entirely too formidable for them, and the practical result is that much time and labor is expended in order to learn to *avoid* the false opening. This was the case in the instance of door *a* in the "Mouse-trap." In maze L, the initial problem assumed a slightly different aspect: the crucial thing was to decide whether or not the long path must be traversed.

In a maze consisting of a series of simple cul-de-sacs, the learning effort tends to extend over a prolonged series of trials, each of which results in a slight addition to the detailed knowledge of the route. As in the type just discussed, the emphasis is upon learning to avoid false openings; but there are many of them, and only a few can be mastered in a single trial. The simplicity of each is a guarantee against the subject's becoming hopelessly lost in the path, and in this way unduly prolonging the trial. The records of Experiment IV indicate clearly the curve belonging to this type of maze.

It should be remembered that our results pointed to considerable freedom for the learner in the matter of the distribution of his time. Hence, it is quite conceivable that one could deliberately decide to learn as much of a simple maze as possible

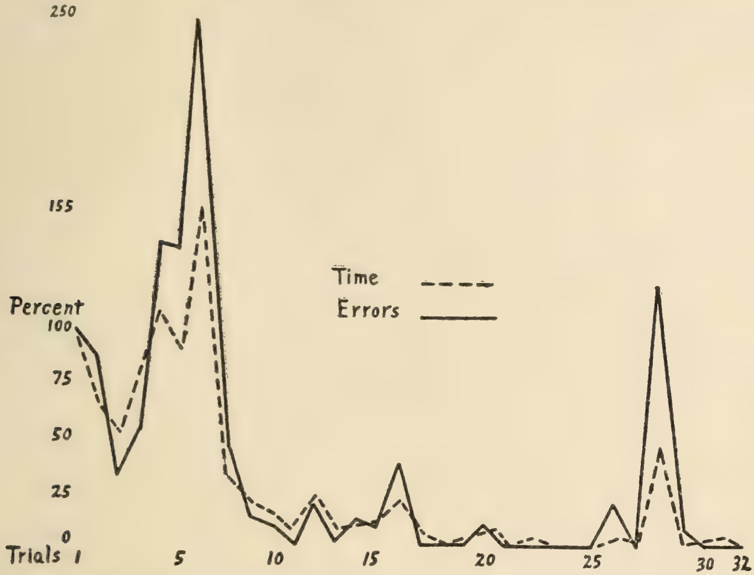

FIGURE 20. Percentage Curve, E. C. P. Normal Maze.

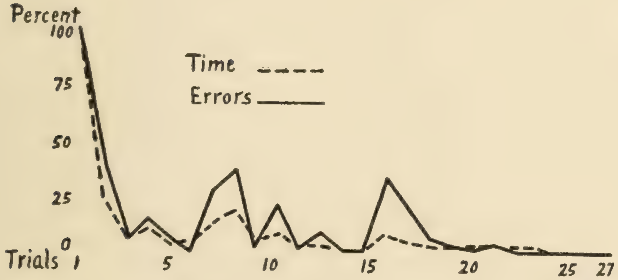

FIGURE 21. Percentage Curve, H. F. A. Normal Maze.

in the first trial, by prolonged exploration. Such a procedure would tend to result in the type of curve that we have just associated with the more complex maze. This method was however not followed by any of our subjects, in the simple mazes, so that it would scarcely seem to be a natural way of learning.

On the other hand, it would be impossible for a learner to distribute his time and errors in such a maze as the "Mouse-trap" in any other way than that shown by our subjects, assuming that the maze presented the same problem to him as to them.

A prime factor that we conceive to be responsible for the sudden fall of the curve at its beginning has just been indicated. All of our records show, however, a general tendency for the curve to drop after the first trial or two, irespective of the scheme of the maze. Hence, another factor must be operative. The explanation of this tendency is to be found in the introspective reports.

The reports show that without exception the net result from the first trial was a knowledge of the general spacial relations. The relation of exit to entrance, the general course of the true path, was acquired by everybody in the first trial. This was especially obvious in the Normal maze, but was sufficiently in evidence in the "Mouse-trap" reports.

The subject had for the second trial, therefore, a skeletal scheme of direction. Assuming that in the absence of detailed knowledge of the maze, he was as likely to enter cul-de-sac 6-9 [page 4] in trial II as in I, in escaping from it, however, in the second trial, this general idea would tend to inhibit him from turning on H. In the first trial, he had no reason to assume that the exit was to the left of this region. As a matter of fact, several subjects testified to their surprise in discovering that the maze extended as far to the left as it did. This applies to every region in the maze, either the path or the cul-de-sac.

It is to be remembered that in the first few trials the learner

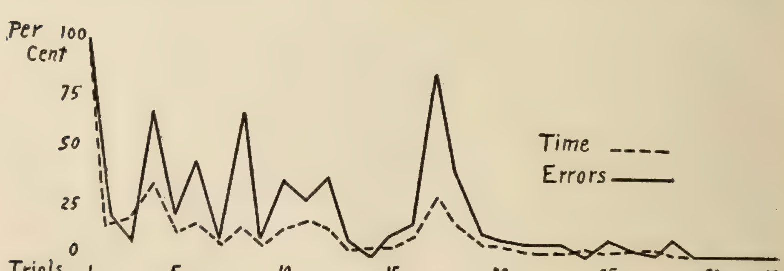

FIGURE 22. Percentage Curve, J. J. T. Normal Maze.

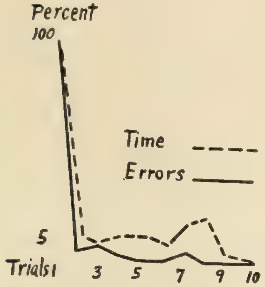

FIGURE 23. Percentage Curve, J. R. A. The "Mouse-trap."

did not recognize H or I as belonging to the true path, as distinguished from 6. The ability to distinguish which of a series of turns had led him into a blind ending was acquired only after more prolonged study of a region. Even a simple situation like 13-14, when the subject entered it for the first time, did not come to him as a cul-de-sac, but as a runway making a right-angled turn. The subject, instead, found himself, after a series of turns, blocked by a blind ending. Hence, in the second trial, his control was the dominant idea of taking any turn which led to the left.

The behavior in the second trial was motivated by this general working scheme, as was also, to a large extent, the third. Specific problems began to be raised in this trial, to be acted upon in the fourth. Since the control for III was practically the same as for II, but more specific and definite, a second decrease in time and error was to be expected; but since the *essential* aspect of the idea had functioned for the second trial, a less pronounced decrease would obviously result. The introspections are in complete accord with these suppositions.

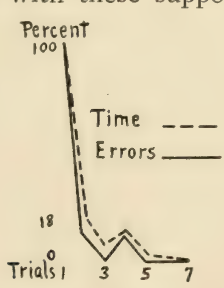

FIGURE 24. Percentage Curve, M. R. F. The "Mouse-trap."

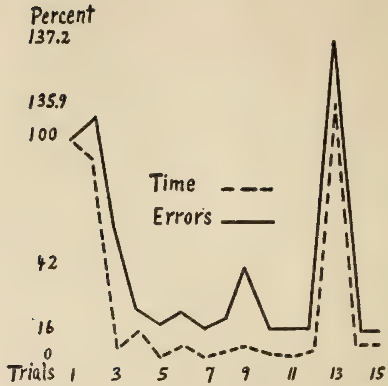

FIGURE 25. Percentage Curve, W. S. H. The "Mouse-trap."

(2) All of the pencil maze curves, and some of those plotted from the park maze records, disclose an irregularity marking the trials III or IV, and a series of fluctuations—the "steeples"—extending over the following half dozen or more trials. There are a number of reasons which account for these irregularities. Some of these may be conveniently grouped together, as constituting a general causal factor.

The general reason why, in the stage of learning represented by the third or fourth trial, and extending throughout the learning, fluctuations should occur, is to be explained by the fact that at this stage the control was rather definitely changed from a general spacial idea to a series of specific control ideas, built up by various factors, each tending to increase temporarily the time and error records. After the third trial, the process of learning the maze resolved itself into a series of individual problems.

Again, the reports from the "Normal" maze indicate the nature of these problems. The learner knew by this time that a rather complex cul-de-sac occupied the right section of the maze. He was interested, therefore, in learning definitely how to avoid it, either by fixing in mind the specific turns in the true path, or the openings in the path. His general scheme had been found inadequate to carry him through this vicinity safely. He was interested, not in working *through* this region, but in traversing it without error. We find reports of the subject's being "hopelessly lost" in this region in the 3-5 trials, that we did not find

in the initial trials. They were lost because their problem had changed, not because their general orientation was less known.

Therefore, at this stage, in ascending path 6, not knowing yet its relation to the rest of the maze, the subject, actuated by a special motive, might: (1) be tempted to explore the region. This being the case, he was as likely to turn into H as to continue on I. A number of errors would result as a consequence. In fact, he would tend to take H rather than I, since his object was to discover the relation of the cul-de-sac to the earlier part of the true path, or the part that had led him into it. (2) The learner might be interested in fixing in memory, or in discriminating in turn the various paths of this portion of the maze. Several of the subjects spent a consderable amount of time in going back and forth between the end of 7 and 9, until the inverted T notion of the situation was established in mind. (3) In the case of being "hopelessly lost" in this region, as the result, sometimes of exploration, sometimes of merely an attempt to get through, a large number of errors were often scored. The subject would resort to an almost pure type of trial and error, in order to find the true path. Even in this, however, he was guided perhaps not completely consciously, not primarily by the general sense of direction, but by the more specific feelings of familiarity and knowledge of this region already acquired. He was more interested in discovering the known part of the true path than he was in finding the exit. Hence, more errors would result when he was lost than in the earlier trials, in the same objective situation. Any of the three procedures just mentioned would tend to raise the curve.

The introspection brought out a number of specific but more or less interrelated explanations of irregularities in the curves during this period of definite maze problems. The first grouping includes those factors intimately involved in an attempt to work out a situation or problem consciously. These factors, or motives for behavior, were: (1) conscious exploration, with the object of studying any particular segment for the purpose of eliminating the non-essential parts of it. (J. J. T. VI, VIII, X, XVII, G. M. F., VI, and all other subjects at times); (2) retracing for

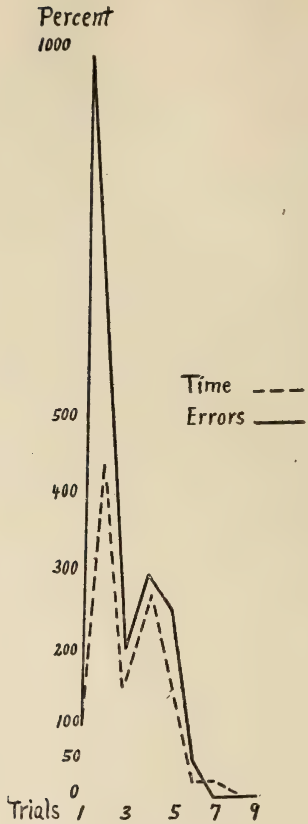

FIGURE 26. Percentage Curve, E. W. J. The "Mouse-trap."

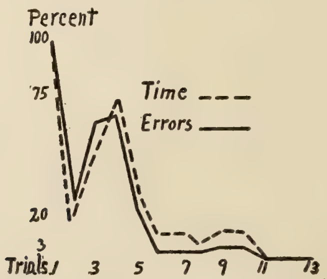

FIGURE 27. Percentage Curve, R. B. O. The "Mouse-trap."

the purpose of fixing in memory a certain segment; (3) misinterpretation of the experiences, incidental to the study of a region. This did not always increase the time and errors. A second group

finds its general explanation in the fact that attention and interest, habit and emotional conditions were in part variable factors. Hence we had reported, (4), distraction of attention, (E. C. P. VII and others), (5) general laxity of attention (M. H. S. H., VI, XII, J. J. T., VIII, M. R. F., XXXI), (6) over-reliance on automatisms, (J. W. H., VIII, XI, XIV, J. J. T., XVII). A third and most common cause of errors was (7) due to the tendency to resort to a trial and error procedure, as a result of fatigue, discouragement, accidentally getting lost, and various conditions. It is not implied that this method necessarily was the cause of more errors than would be the attempt on the part of the subject to maintain a more conscious attitude.

The irregularities occasioned by the development of specific problems were necessarily involved by them. No study of a situation could be possible without costing an expenditure of time, and of errors, under the marking system employed in the experiment. The tax on memory was too great to permit certain regions to be learned without frequent retracing through them.

Increases due to the other factors seemed practically as unavoidable. The demand upon the attention and memory was rather severe during the learning period, and assuming even the possibility of the subject maintaining a constant amount of attention during the trials, the problem of its distribution was a vital one. To the extent to which the subject concerned himself with the attempt to keep in memory a section just learned, throughout the remainder of any trial he must necessarily be somewhat lax in attending to the situations encountered.

The reasons specified account for irregularities; they hardly explain the fact that these irreguliarities graphically should take the form of steeples. Or, in terms of the quantitative results, while they account for a sudden increase in time and errors at any given instant, they do not explain why this was invariably followed by a corresponding decrease in the next trial.

The fact that each new trial meant a "fresh start" was a strong factor tending to safeguard the subject from carrying into any trial the bad effects of the previous one. Accidents in a trial due to laxity of attention, or reliance upon habit, seemed to put

the subject on his guard for the next trial. The emotional effects were as a rule not carried from one trial into the next. At the same time, however, a problem generally extended over a series of trials; and to the extent that no special progress was made in any one trial, the records tended to be similar for the series. Hence the curves show the combined effects of the two factors.

III. SUMMARY OF RESULTS AND CONCLUSIONS

A. The nature of the learning process

1. The human adult in learning a maze employs the conscious processes of discrimination, memory, etc., in order to build up an ideational control. Unconscious learning of segments which represent any degree of complexity is practically impossible.

2. Two chronological stages were in evidence in the conscious part of the learning. The subject was guided in the first few trials by a general scheme of direction, gained when the exit was attained for the first time. He then found that in order to perfect his route a number of separate segments, presenting special problems, must be studied.

3. Difficulties were offered mainly in the form of memory or of analysis and interpretation. A maze whose cul-de-sacs were simple primarily taxed the memory; one in which the formations were intricate called for more active analysis.

4. The immediate reaction upon the maze experience is perceptual in its nature, and simple formations are immediately and easily analyzed. A complex formation, on the other hand, calls for an interpretation of the difficult segment. It is in the elaboration of this interpretation that higher mental activities are elicited.

5. The rational processes reported were unsystematic and seemingly futile. Adequate interpretations were suggested to the learner as the result of prolonged exploration, rather than reasoned out. Cues which logically should be utilized for correct inferences were disregarded, and ideas were acted upon in an uncritical manner until they were proven by trial to be incorrect. The explanation of the meagre attempts at reasoning is to be found in the fact that the learner had no past experience to apply

to the situations, and in the fact that he was unable to select his data—maze paths must be traversed in order.

B. Specific functions involved in the learning

(a) Sensory discrimination: 1. Sensory discrimination in the pencil mazes was made possible by, (a), the feeling of arm position; (b), the length and direction of arm movement, in any given run-way; (c), the sense of tactual projection, at the point of the pencil.

2. A series of specific tests failed to warrant any assumption of correlation between ability to learn mazes and proficiency in sense discrimination.

3. A number of supplementary tests proved that transference from one set of muscles to another is easily accomplished. That is, the left arm can be used without loss of efficiency after the right arm has been employed in the learning; and wrist and finger movement may be substituted for arm movement.

4. While the physical technique called for in the "Mousetrap" seemed radically different from that demanded in the pencil mazes, the learning process seemed to be essentially the same. The learning was perceptual and ideational, rather than sensory.

(b) Imagery: 1. The subjects represented a rather inclusive series of image types and combinations. Each individual employed his peculiar image equipment for all mazes.

2. Objective tests purporting to check up reports on imagery were in the main unsuccessful; they did however serve to convince the subject of the accuracy of his introspections on imagery.

3. We were unable to make any correlation between the type or combination of image processes used with efficiency in maze learning. In addition to the absence of obvious correlation, the fact that different subjects used the same form of imagery in different ways made comparison impossible.

4. Some reports were found which indicated that those relying upon kinaesthetic image processes tended to rely more upon motor habit than the other subjects.

(c) Attention and Habit: 1. Attention, in learning any maze,

was distributed in a three-fold way: it was concerned with, (a), the actual experience while traversing any section; (b), the "trail behind"; (c), an anticipation of the turns to come. This, roughly speaking, represented a chronological sequence during the learning of a maze.

2. To the extent that the subject was able to disregard the immediate experiences, he tended to rely upon habit to carry him through the passages.

3. Habit appeared early in the trials, with the pencil mazes, in connection with the segments first learned, while other segments were requiring study. Complete automaticity was not reported for an entire trial with any subject.

(d) Memory: 1. Memorizing was recognized as the chief difficulty only in connection with the simple mazes; with the other mazes, the difficulty was one of analysis.

2. Experiments designed to test the ability to memorize—i.e., with intricate maze paths lacking cul-de-sacs—failed to establish correlation of ability to memorize with proficiency in learning actual mazes.

3. In addition to the difficulty of retaining knowledge just acquired, while actually traversing a maze, some of the subjects complained that the intervals between trials—in Experiment I—were too long. The reports indicated that an equal distribution of interval time is not necessarily the most efficient distribution.

(e) Illusions: 1. In practically all the mazes, relative lengths of passages, and the size of angles, were wrongly estimated. Not only were the proportions distorted, but the size of the maze as a whole was generally over-estimated. This was especially in evidence with the "Mouse-trap."

2. Illusions were not as a rule productive of bad results, inasmuch as they did not disturb the notion of the turn sequences. In one or two cases, however, a different estimation of the length of a passage from the one usually made suggested to the learner that he was astray, and errors resulted.

C. The learning method

1. A certain amount of active memorizing was necessary in

order to learn the simplest mazes, and a genuine study of maze situations was called for in the complex mazes.

2. The nature of the mental effort depended upon the complexity of the maze, not upon the volition of the learner. The learner was able to vary the temporal distribution of his effort to some extent.

3. Pure "trial and error" and "pure" reasoning, or even "ideational learning," are equally inadequate terms to characterize the leaning method.

D. The learning curves

1. The obvious features of the curves were, (a), the rapid initial descent; (b), the steeples.

2. Two factors enter into the explanation of the initial descent: First, a maze whose cul-de-sacs are intricate necessitates an elaborate expenditure of time in learning to avoid entering the cul-de-sac openings. Secondly, in any maze, the notion of the relation of exit to entrance, and the general idea of the course of the true path, acquired in the first trial, enable the subject to eliminate purely random "try-outs," and thus to center his activities.

3. Steeples are associated with the second phase of the learning, in which specific problems are attacked—these problems call for systematic exploration, etc., which temporarily prolongs the time. Steeples are also due to carelessness, over-reliance upon habit, etc.

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On the Psychophysiology of a Prolonged Fast

By

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EXPERIMENTAL CONDITIONS

In the spring of 1912 Agostino Levanzin, a lawyer by profession came from Malta to undergo a prolonged fast at the Nutrition Laboratory of the Carnegie Institution of Washington in Boston. It was deemed advisable, in order to have as complete an investigation as possible of all the conditions during fasting to supplement the physiological by mental tests. It is the purpose of this paper to present the results of the latter.

Levanzin has for a number of years been interested in the subject of fasting, believing that most human ills can be cured by abstinence from food for a long period of time. He had already made one fast of forty days. It was his claim that during that period all his mental faculties so increased in efficiency, that he could hear, see, smell and think better and that on the 26th day he was able to plead a case in the law court. His ostensible purpose in coming to America was to substantiate, if possible by strict scientific methods his own casual observations. It is desired to emphasize this attitude in order to convey an idea of the keen interest which he showed during the tests and the willingness with which he attempted to fulfil all conditions, a cooperation the thoroughness of which might have been doubted in the case of a man fasting merely for pay.

Before his first fast he weighed about one hundred and eighty pounds and after the fast 140 pounds. When he arrived at the laboratory his weight was 134 lbs., at the end of the fast 106 lbs.[1] He was a man of 40 years of age, of medium height and slender. When not in conversation his manner was languid and it is perhaps due partly to this that he seemed to lack physical strength and vigor. In temperament he is of the decidedly emotional southern type, sensitive, quick to anger, loquacious, credulous and fertile in imagination. This last characteristic is probably responsible for the fact that the unusual appeals to him. Once having espoused a cause or entertained an idea he holds to it

[1] There was an almost steady decrease of about one pound a day.

tenaciously. He is a man of a few fixed ideas or complexes which form the basis of his mental life.

L. arrived at the laboratory on the afternoon of April 10th. He took his last meal on the evening of April 13th and his fast was considered to date from the morning of the 14th. Absolutely nothing but 750 cc of distilled water daily passed his lips during the thirty-one days. He lived on a balcony in the laboratory, slept at night in the calorimeter and was watched constantly. His luggage was examined on his arrival and all of his mail was opened at his request in order to preclude every possibility of his receiving a stimulant through those sources. His daily program consisted of tests by different specialists and varied but slightly. His leisure he spent in reading and writing and receiving guests. Unfortunately the weather for a good part of the time was unfavorable, but when possible he went on the roof for an hour or took a ride in an open carriage with an attendent.

The tests herein described lasted from April 11 to May 15th inclusive.[2] Food was taken on April 10th, 11th, 12th, and 13th and again on May 15th. The intervening 31 days were fast days. The psychological tests were made at five p. m. each day and lasted one hour. During the half hour before the tests he rested.[3]

From the above it is seen that the psychological tests were made under as nearly as possible ideal conditions. Alone from the fact of the complete control of diet and occupation the tests seemed worth the attempt, for it is seldom that psychological experiments can be conducted over such a length of time under such constant conditions. Perfect as they were, however, one factor important to mental measurement was found to vary, that is the mood of the subject. As far as L's willingness to cooperate is concerned there was nothing to indicate to the experimenter a change in this attitude or that his general interest in

[2] An idea of his intelligence and interests may be obtained from the association reactions. See appendix II.

[3] The tests on April 11th were tentative and are not included in the curves.

the work relaxed at any period of the series. On the other hand there is no doubt that he was happier during the first days, rather depressed and silent in the middle and somewhat irritable and excitable toward the end, although this irritation was at no time directed toward the tests. The greatest depression occurred after a prolonged continuation of bad weather and very much decreased after he was able to go out in the air. He was also much happier after having received visitors. He himself remarked that the monotony of the program was the most difficult thing he had to endure. As to his physical condition he made few complaints. He felt well throughout and insisted that he had no sense of hunger, not even during the first days.[4] The only discomfort of which he spoke was the coated condition of his tongue and the unpleasant taste in his mouth. It was his idea that the fast should continue until this disappeared and it was for this reason that he was loath to break his fast on the 31st day.[5] Although he seemed more feeble toward the end of the fast and gave one the impression of a man convalescing from a weakening illness, yet he was always able to walk without assistance and at no time was it necessary to omit or alter a test through lack of strength on his part. On May 15th, the day he broke his fast, he suffered severe colic induced by the food he ate and although tests were made, the conditions were most unfavorable. It had been planned to continue the examination for several weeks longer, in as much as such tests would ob-

[4] This is contrary to the experience of most fasters. W. B. Cannon and A. L. Washburn (An Explanation of Hunger. Am. Jour. of Physiol., 1911-12. p. 441) describe the feeling of hunger as follows: "Hunger . . . is a dull ache or gnawing sensation referred to the lower mid-chest region and the epigastrium. It is the organism's first strong demand for nutriment, and, not satisfied, is likely to grow into a highly uncomfortable pang, less definitely localized as it becomes more intense. He further states (p. 442) : There is abundant evidence, however, . . . that during continued fasting hunger wholly disappears after the first few days." Professor Cannon has recently informed the author that from what certain fasters have told him he believes that sensations of hunger may be absent from the beginning; that in fact some people may never have the sensations of hunger as just described.

[5] Thirty days were considered sufficient for the physiological tests and he was allowed one day more to excell Succi's record.

viously be of inestimable value for comparison with the fasting tests. Unfortunately that was quite impossible under the circumstances and an entire year elapsed before further records could be obtained.

Several factors influenced the selection of the tests. In the first place the time was limited. There was only one hour daily available and it seemed advisable to arrange for as many tests as possible during this hour in order to obtain a good mental picture. It was therefore necessary to choose short tests and also those requiring the minimum of effort, as one test had to follow the other without pause for recuperation. For example prolonged tests for fatigue would have been of great value but they could not be considered. In the second place the fasting began a few days after L's arrival and little time could be devoted to preliminary trials in order to obtain the best combination and the program once arranged could not be fundamentally changed.[6] After consultation with Professor Dodge a series of tests were selected. A few days' experience, however, showed the necessity of several alterations, and the revised program was as follows: 1. Rote memory for words, 2. Tapping test, 3. Strength test, 4. Tactual Space threshold, 5. Touch threshold, 6. Free association and reproduction reactions, 7. Association reactions, genus-species, 8. Association reactions; noun-verb, 9. Cancellation test, 10. Hand-writing,[7] 11. Visual acuity, 12. Memory for words after 55 minutes. Later the touch threshold, which was taken on the under part of the lower forearm with a von Frey hair, was discontinued on account of the impossibility of obtaining reliable results in a short period of time. The association reaction genus-species was also omitted through difficulty in finding sufficient reaction words of equal simplicity. In addition to the tests Levanzin was requested to describe all the dreams he had on the previous night.[8] This was given before the visual acuity test. All the tests with the exception of that of visual

[6] A few minor changes were introduced.

[7] A superficial examination of the daily records revealed no change. A systematic examination of the data has not yet been made.

[8] See appendix I.

acuity were made in a small room free from disturbing influences.[9]

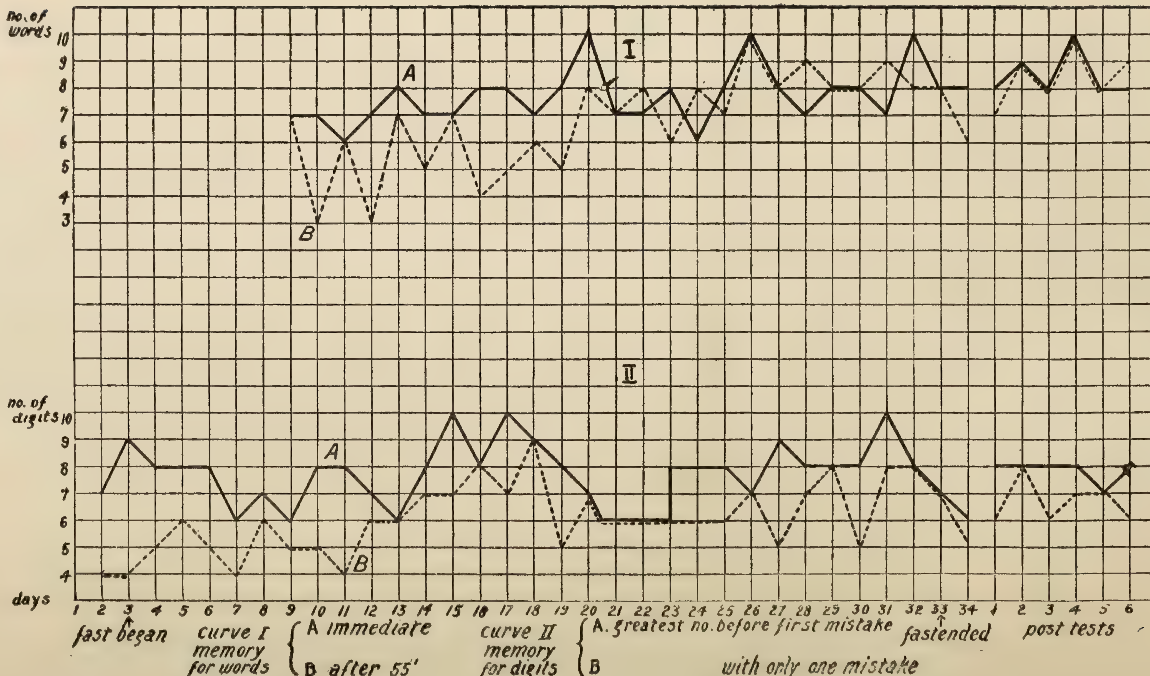
The general conditions of the experiments and the nature of the tests having been described, each test will now be treated separately, first as to the particular conditions and second as to the results.

Memory for Words

Ten one syllable words were chosen and these were read twice to the subject, who recalled as many as possible immediately after the second reading. After fifty-five minutes the subject again attempted to recall these words.

From the curves it will be seen that there are marked fluctuations, a circumstance which is always met with in mental tests and which will be found in all the curves. It will therefore be only possible to speak of general tendencies throughout. In the curve for immediate rote memory (IA) it will be seen that the poor record made on the eleventh day (the third day of the test) only occurs once again and that on the twenty-fourth day, while a perfect score of the ten words was made three times and all of them during the last two-thirds of the fast, so that although the initial records occur frequently toward the end, yet the curve as a whole shows a slight general improvement, but so slight that much significance cannot be attached to it. The curve (IB) indicating the amount of retention after 55 minutes, on the other hand shows a more or less steady improvement until near the end of the series and even when these last trials are included the general tendency of the curve is decidely upward. In four instances and these all in the last two-thirds of the series the retention curve crosses the rote memory curve, which means that on these days the retention after the lapse of almost an hour was better than the immediate memory. Levanzin, upon being questioned was emphatic in his assurance that he never thought of the words in the interim, so that this relative improvement in retention was not due to any conscious repetition during the pause.

[9] It is much to be regretted that time and conditions prevented tests for the thresholds of audition and smell.


no. of words
I
A
B
II
no. of digits
days
fast began
curve I memory for words
A immediate
B after 55'
curve II memory for digits
A. greatest no. before first mistake
B with only one mistake
fast ended
post tests

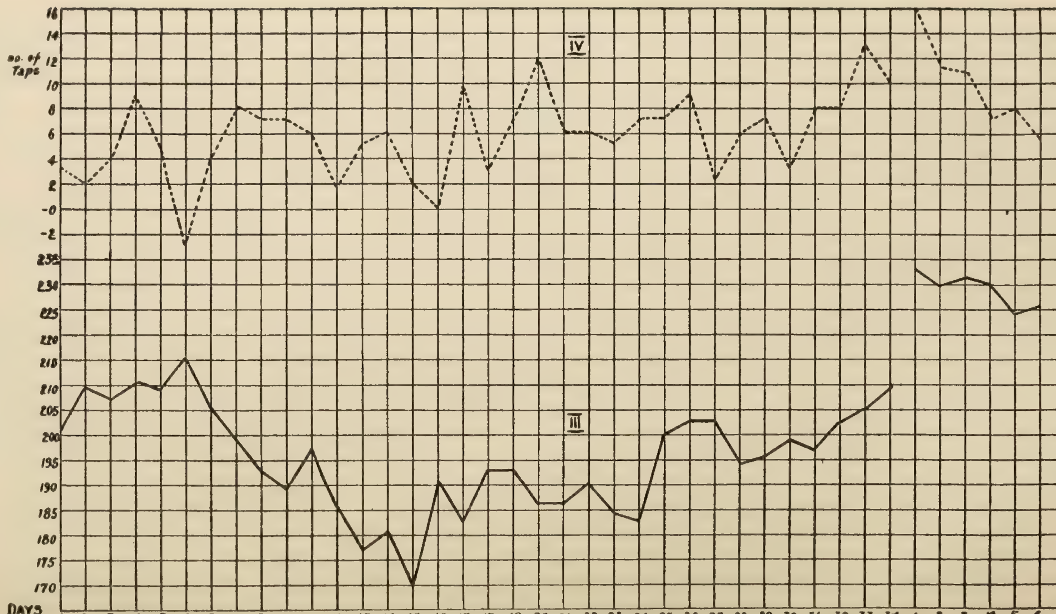
Tapping Tests

The instrument used was similar to the tapping-board described by Whipple.[10] It consisted of a board 12 cm. square covered with aluminum. This metal is not very well adapted for the tapping-board, but it was selected for its lightness, it being thought quite probable that the tests would have to be made toward the end of the experiments with the subject lying down and the board resting on his chest. The stylus also had an aluminum point. The records were taken on a kymograph. The tapping lasted for thirty seconds and periods of ten seconds were marked off on the records. The subject being left handed used that hand. As he was over sensitive to cold during the fast he wore, beside a heavy woolen undershirt, a heavy dressing-gown, which added to the weight he had to lift. Neither the hand nor arm was allowed to rest on the table during the tapping.

The curve (III) shows a gradual improvement for the first six days when the maximum of the series—215 taps or about seven taps per second—was reached. The curve then descends for the next nine days when the minimum of 170 taps was reached. From this point to the end of the series there is a rise to a point just below the maximum. This rise is not, however, gradual, but consists rather of two plateaux, one of nine the other of seven days separated by decided jumps and followed by a gradual but very marked end spurt of four days.

The initial improvement can well be due to practice in using those particular sets of muscles, combined with increasing familiarity with the work. This same rise also occurred in the dynamometer tests. The drop, however, begins much sooner than in the dynamometer tests. In fact it ends in the former where it begins in the latter. One can therefore hardly say that it is a matter of muscular fatigue. The first explanation to suggest itself is a lessening in interest, and this is strengthened by the fact that the drop occurs at that time when he was most affected by the monotony of the routine work. In this test less

[10] Whipple's Manual of Mental and Physical Tests. p. 101.



Curve III = no of taps in 30" Curve IV = difference between 1st and 3rd 10"

fast began

fast ended

depends for improvement upon the increase in muscular power than in the dynamometer tests, the main factor being the rapidity of action. We know that the rate of the reaction time is greatly affected by changes in attention and it is probable that the betterment in the muscular control, which we may assume from the results of the dynamometer tests did occur, was insufficient to offset this loss of interest. The results of the last days confirm this assumption, for here we undoubtedly have the effect of interest in an end spurt, which, notwithstanding muscular fatigue which was undoubtedly present at this time, brings the curve back to a higher level.[11] In regard to the two plateaux referred to above, it seems plausible to infer from what we know of the causes of plateaux in the learning process in acts of skill that these sudden rises to new levels are due to the learning of some new method or short cut. Here the most obvious short cut is the lessening of the height of the stroke.

An examination of the difference curve (IV), which has been obtained by subtracting the results of the first ten seconds from that of the last ten, still further confirm the assumption of a wavering in interest. There is a gradual increase in the amount of this difference, which indicates fatigue. This increase is particularly marked toward the end when the records are improving, which means that the improvement is caused by a spurt during the first ten seconds.

In general it may be said that although initial lack of interest[12] and later muscular fatigue played a rôle, both factors being directed toward a decrease in the amount of work, yet the will impulse toward the end was sufficiently great to bring the curve back to its initial level and almost to its maximum.

[11] This is an error which is bound to occur with this form of tapping board. The writer has, therefore, recently constructed a board which regulates the height of the stroke, thus making it a constant factor.

[12] Against this suggestion is the fact that other tests did not show this lack of interest, but it is quite possible that the interest varied with the different tests.

curve V · STRENGTH TEST
LEFT HAND
curve VI · difference between 1st and last three trials

Strength Tests

These tests immediately followed the tapping tests. The subject stood and received the dynamometer, one of the Collin type, from the experimenter, and pressing it returned it to the experimenter. The record was noted and the instrument returned. The interval between trials was about one second. Ten trials were made with the left hand followed by ten trials with the right.

Both in the right (VII) and left hand (V) curves there is an initial falling off, which is more marked with the former hand. The latter, however, continues to fall to the 11th day on which day it takes a decided drop, while the former declines more gradually to the ninth day, when it reaches its maximum. Both curves then rise to a maximum, which is reached by the left hand on the 16th day and by the right hand on the 12th day (the record of the first day not being considered in speaking of this maximum). The curves then fall, the left much more than the right, especially in the middle of the series, the former reaching its minimum on the 31st day. Both curves show a slight end spurt. This is, as a glance at the curve will show, merely a rough picture, there being decided rises and falls throughout.

In interpreting the curve it must be remembered that the left hand is the practiced hand and it can therefore be assumed that the muscles of that hand are the stronger. In fact the results make this more than an assumption, for the record of this hand is at all times decidedly better than that of the right hand. The initial falling off is what one must expect when the subject is not accustomed to the particular muscular exercise. There is a great exertion at first and the muscles, skin and subcutaneous tissue feel the usual strain for several days. Those muscles least accustomed to exercise are the most effected. It is for this reason that the right hand record drops more than that of the left hand. Then the muscles gradually recover and the effect of practice begins to appear. Acting against the practice is

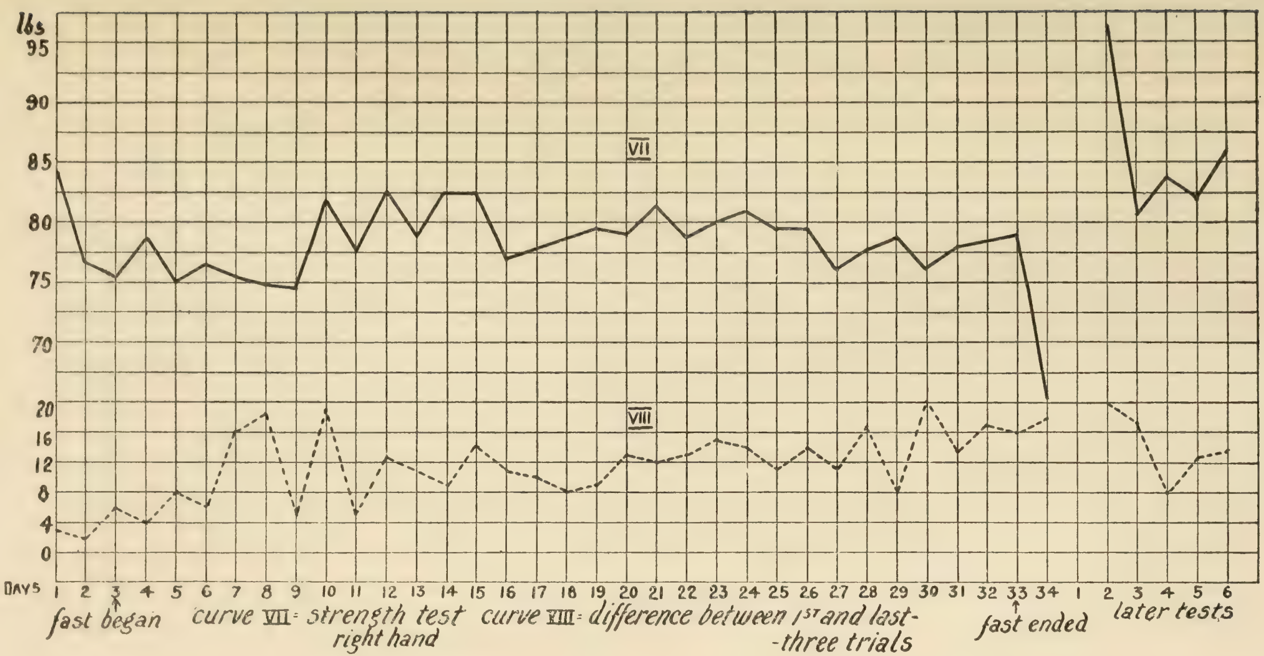
the increasing fatigue. The right hand being the unused hand gives practice more chance for its influence and although fatigue never allows the curve to reach its first day's record, yet the drop which soon begins is much more gradual, as has been pointed out, than it is with the left hand, where the effect of fatigue is more prominent.

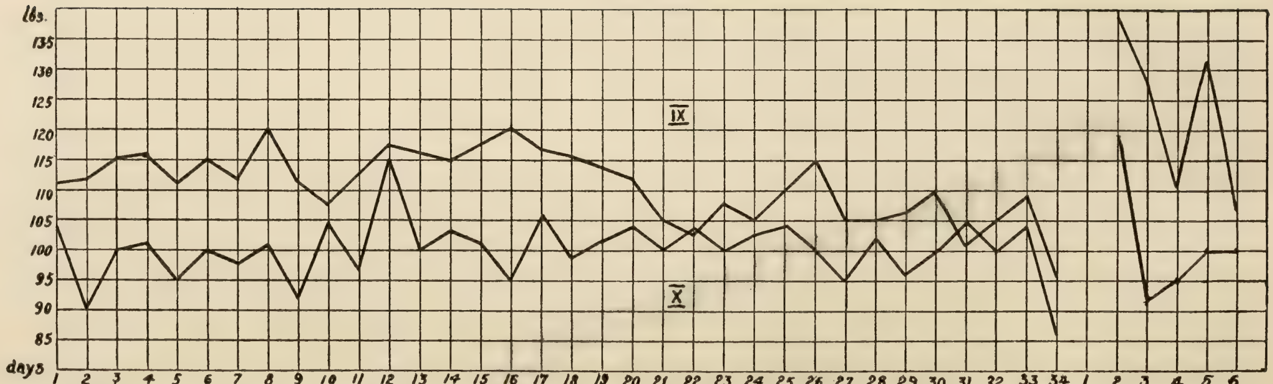
The difference curves (VI, VII), which were obtained by subtracting the average of the last three records of each day from the average of the first three helps to strengthen the conclusions just drawn. The rise of the difference curve at the same time as the fall of the main curve, means, of course, increasing fatigue, which shows itself in a greater and greater drop toward the end of the daily series. This rise in the two difference curves is relatively about the same, which means that the daily increase in fatigue is relatively the same for the two hands. Further, if we glance at curves (IX, X), we find additional indications in the same direction. This curve is plotted from the first of the daily series of ten trials. This trial is least effected by fatigue and shows therefore the greatest influence of practice. Here there is a gradual rise for the right hand until next to the last day, while the curve for the left hand begins to drop where it should according to our analysis.

In general we may therefore say that fatigue appears in both hands early in the series. The curve for the left hand drops far below the record of the first few days. The curve for the right hand shows less drop due to the greater influence of practice, so that the two curves tend to approach one another.

Tactual Space Threshold

A pair of dividers with wooden tips were used as an aesthesiometer. The threshold was found on the volar side of the forearm, about four inches from the elbow. The points were applied on either side of a red ink dot which was made on the arm on the first day and renewed when necessary. The method of minimal change with ascending and descending series, was employed.


lbs
95
90
85
80
75
70
20
16
12
8
4
0
DAYS
VII
VIII
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26 27 28 29 30 31 32 33 34 1 2 3 4 5 6
fast began
curve VII = strength test right hand
curve VIII = difference between 1st and last-three trials
fast ended
later tests



fast began   curve IX strength test initial daily record left hand curve X-right hand   fast ended   later tests

Five trials excluding one-point "vexier" trials were made at each distance. Four correct out of five was considered the threshold.[13]

For the first few days the curve (XI) keeps the high level of 7 cm. On the seventh day there is a drop to 5.5 cm., then a slight rise to a level of 6 cm. and a high threshold of 6.5 cm. on the 14th day followed by a fall to the minimum of 5 cm. on the 22nd day, which minimum is again reached on the 26th and 30th days. The final days show a rise to 6cm. The decided drop on the seventh day may be due to adaptation to the experiment, which in this instance means the adoption of a definite and clear criterion of discrimination. The drop in the middle of the series after a more or less constant level may be due to a similar cause, that is a change to a better criterion. The rises in the latter part of the curve are never as great as those of the first part, although on the last day the curve again reaches 6.2 cm. This threshold had to be placed at 5 correct judgments as there was a jump from 3 correct judgments. This makes the threshold probably too high. If we omit the first day and compare the average of the period from the 7th to the 20th day with the average of that from the 21st to the 34th day we find a difference of .4 cm. in favor of the latter period. We may say then in general that there is an improvement, although very slight in the discriminating process, but that there is no end spurt, which latter from the very nature of the process under investigation is not to be expected.

Rote Memory for Digits

The usual rote memory test was employed. Increasing series of digits beginning with four digits were read aloud once by the

[13] It had been intended to call three out of five the correct threshold, but this was not found feasible. The threshold is probably too high, but for the present purpose, where the change and not the absolute threshold is being investigated this does not matter.

The curve shows no record for the 4th, and 5th days. The experimenter was absent on these days and the physican, who kindly volunteered his services, did not deem himself sufficiently skilled in this particular test to undertake it.

experimenter to the beat of a metronome with one second intervals and were repeated as far as possible by the subject. The combinations of digits varied daily.

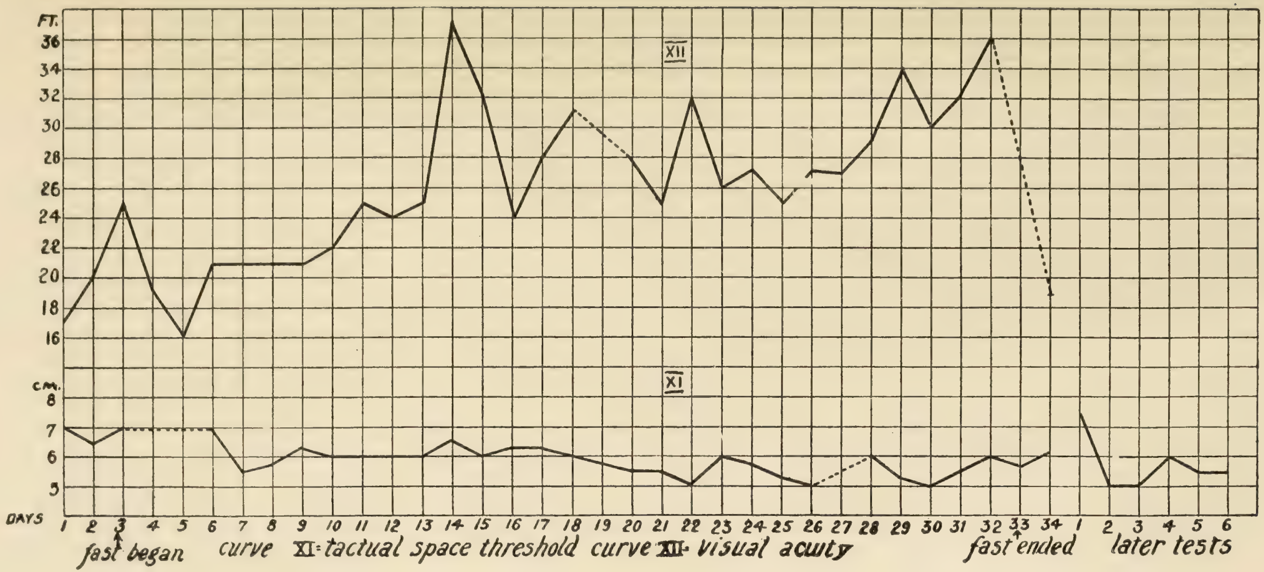
Curve II A is obtained by taking the last series that contains only one mistake, curve II B by taking the number which immediately precedes the one containing the first mistake. Curve A, which gives a picture of the rote memory process shows two apexes of maximal value near the middle and another on the 31st day. There is, however, a very low minimum in the second half of the curve and a decided drop from the maximum of the 31st day. One can, therefore, hardly speak of an improvement. The most that can be said is that the subject was, toward the end of the fast, again able to reach the maximum record of 10 digits obtained near the middle of the series. We see from the curve B that on the 3rd day a mistake was made at four digits, yet the retention is 9 digits; on the 11th day a mistake at four digits and a retention of eight, etc. It seems fair to assume from these results that curve B represents in a rough manner the degree of attention. It is only inattention that can produce results like the above. Curve B shows a decided rise to the eighteenth day, when it reaches a maximum and although it follows a lower level from this day it never reaches the minimum of the first third of the series. One may therefore say that there is an improvement in the state of attention, at least for this experiment, as the fast progressed.

Association Tests

The free association experiments consisted of the daily presentation of a list of twenty words which were selected principally from the lists prepared by Woodworth and Wells[14] and with the exception of the list of May 9th, which was a repetition of that of April 11th they were all different.[15] Several days after the tests were begun it was thought advisable, in order to

[14] Association Tests, Psych. Monog. vol III, 5, 1911.

[15] The lists will be found in appendix II. In a few instances the same word appears in two lists.

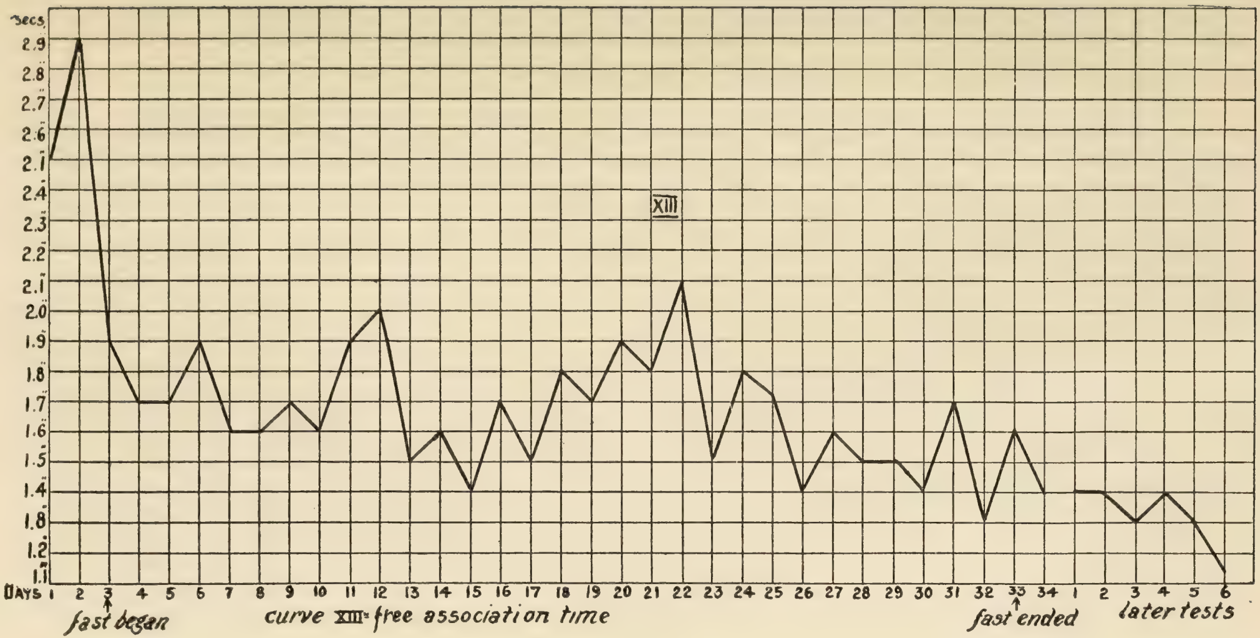

FT.
36
34
32
30
28
26
24
22
20
18
16
C.M.
8
7
6
5
DAYS
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26 27 28 29 30 31 32 33 34 1 2 3 4 5 6
XII
XI
fast began
curve XI = tactual space threshold curve XII = visual acuity
fast ended
later tests

make the lists as uniform as possible, to have them composed of an equal number of verbs, concrete nouns, adjectives and abstract nouns, in the order given. This arrangement was adhered to from April 18th to the end of the tests with the exception of May 9th. The words were read aloud by the experimenter and the time taken with an ordinary stop-watch. The reproduction experiments followed these with only a pause of a minute. Although the subject was told that he need not repeat the same word, if it did not come at once, yet there is little doubt that his efforts were always directed toward that end. Levanzin had a good command of the English language although it is not his native tongue. At times, however, he had difficulty in finding the word he wanted. In such cases, he made a jesture as soon as the idea came to him and the watch was snapped at that time rather than when the English word was found. This method of procedure was not often necessary and it seemed a legitimate means of balancing the slight disadvantage he had as a foreigner. A reserve list was prepared upon which to draw when he did not understand the word of the main list.

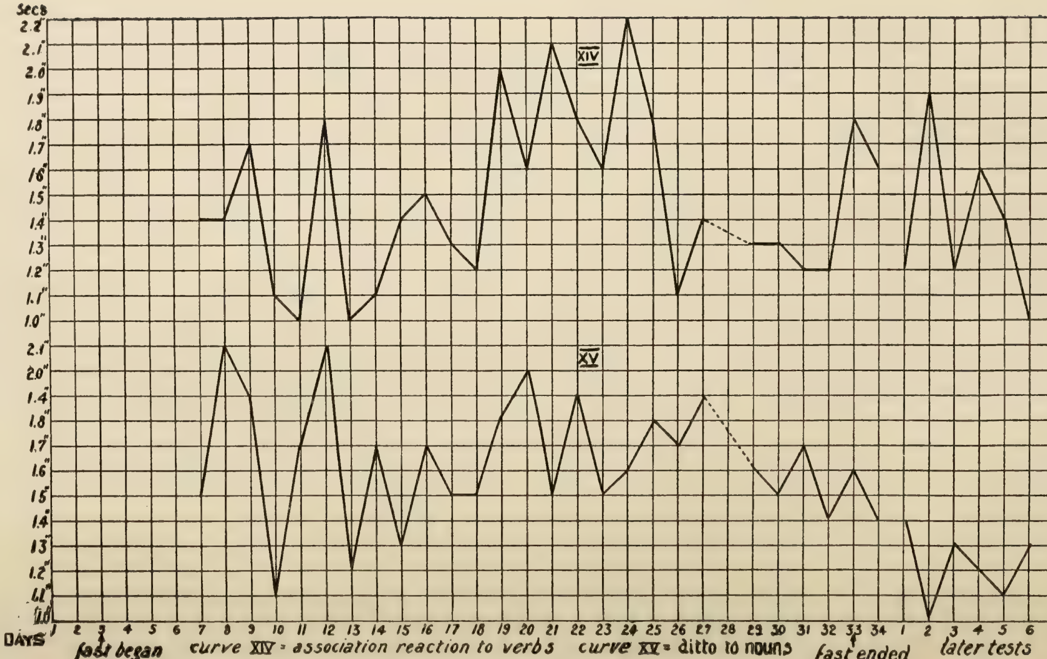
The curve (XIII) is plotted from the daily average. The average was used in order the better to include the influence of the long times, which might very well be of importance in these tests.[16] The few exceptionally long times, such as 20 seconds, which may have been caused by emotional complexes, were not included.

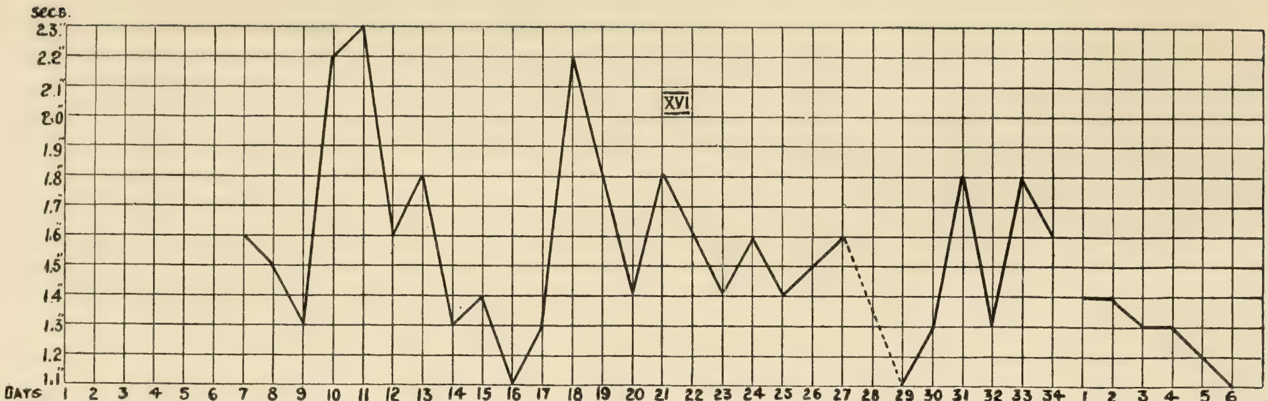
The curve begins with very long reaction times. Levanzin had never performed such tests before, so that the sudden drop on the 3rd day must be attributed to the practice improvement, which at this early stage could very well be sudden and of considerable amount, rather than to the fact that it is the first day of the fast. From this point the curve descends with a few breaks to the 15th day, when it reaches 1.4 sec. It then rises to the 22nd day when it reaches the maximum (if we do not consider the first few days) and then falls to the end of the series. On the second from the last day it reaches the minimum

[16]The median which was also calculated gave the same general curve.

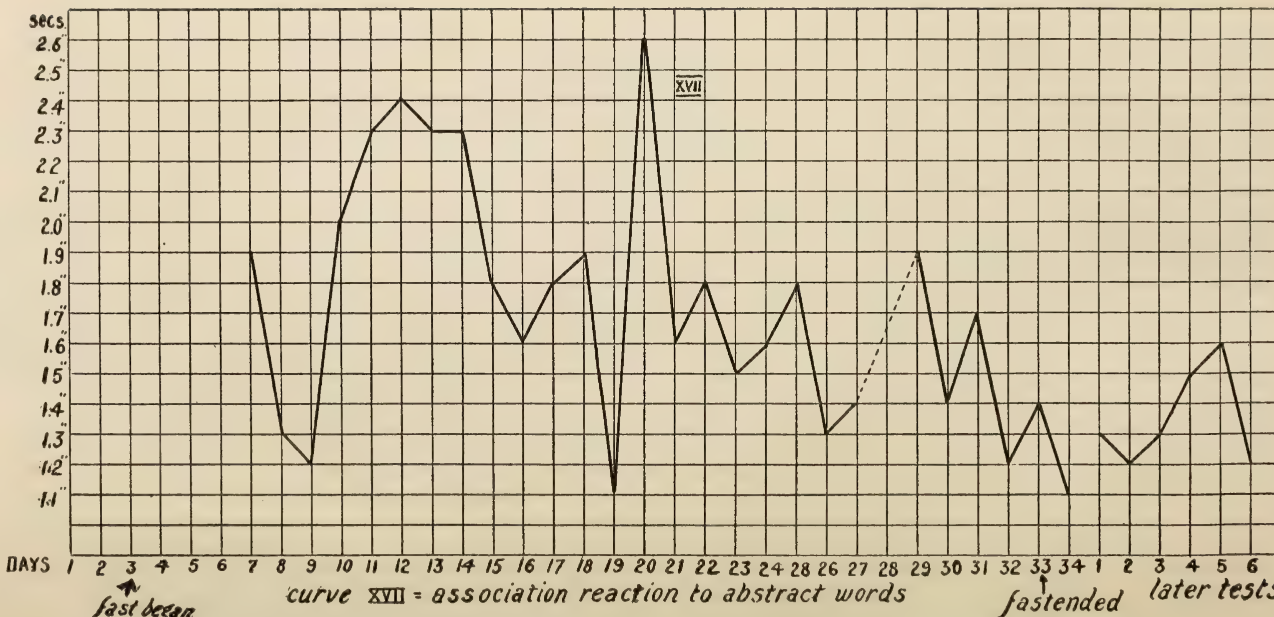


curve XIII: free association time

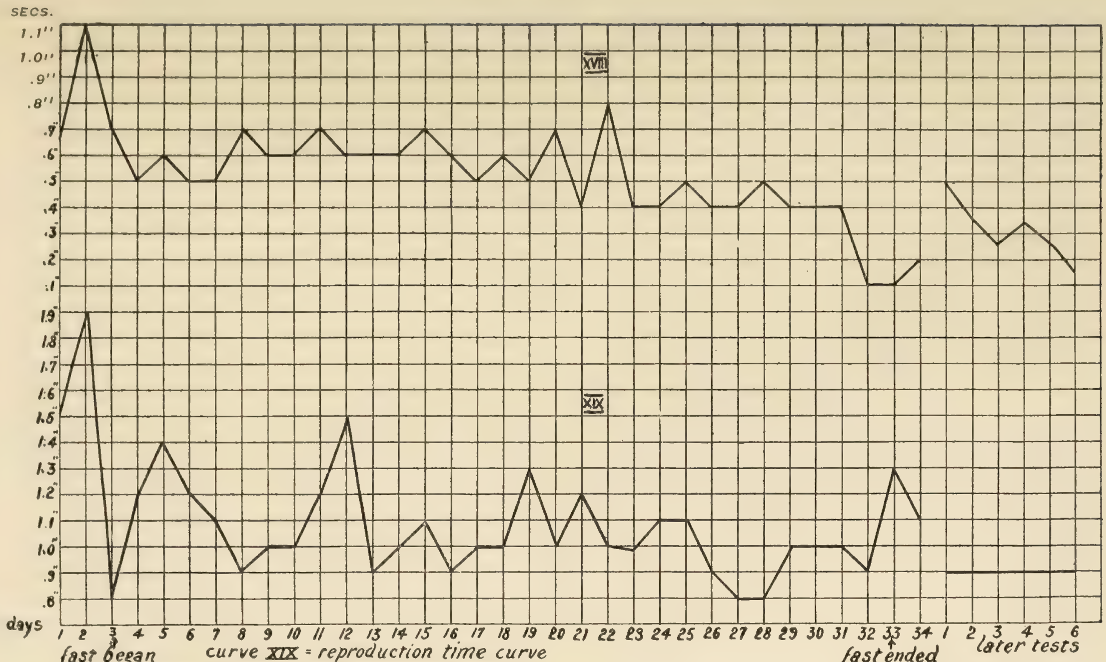

Secs
XIV
XV
DAYS
fast began
curve XIV = association reaction to verbs
curve XV = ditto to nouns
fast ended
later tests



fast began curve XVI = association reaction to adjectives. fast ended later tests



curve XVII = association reaction to abstract words



curve XIX = reproduction time curve
curve XVIII = M.V. of free association time

of 1.3 sec. Also the record of 1.4 sec. is obtained three times in the second half of the series. If we include the first few days it can be said in general that there is a very decided betterment in the association times; and even if one calculates from the third day there is an appreciable drop. Especially interesting is the almost steady improvement shown in the last third of the curve.

In order to analyse the curve further, separate curves (XIV, XV, XVI, XVII) have been plotted for each of the four categories of stimulus words. It must be remembered that these curves begin on the seventh day, when this division into separate categories was first made. In consideration of the fact that the daily average is obtained from only five reactions too much importance must not be attached to sudden daily falls and rises, such as in the abstract series on the 19th and 20th days and in the adjective series on the 18th day, etc., but rather the convex shape of the verb curve, the rise in the middle of the noun curve, etc., must be considered.

It is evident that the rise in the main curve about the 10th to 13th day is caused largely by the noun curve and that the relatively greatest improvement at the end of the curve as compared with the beginning is in the abstract curve. On the other hand the verb and noun curves have several low averages in the beginning that were not reached again. In fact it is hardly possible to say that either of these curves show general improvement, certainly not the noun curve. An examination of the daily fluctuations in the curve shows that this becomes less as the tests progress.

The curve (XVIII) for the m.v. of the main curve shows a decided improvement as the fast progresses with a very low level on the last three days.

The reproduction curve (XIX) follows the tendencies of the association curve. There is the initial drop and many more high peaks in the first two-thirds of the series. If it were not for the rise on the last two days the general betterment would be more marked. The reactions were, on the whole rapid, averaging about 1 sec. and dropping as low as .8 sec. As the number of false

Days	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
	Fast began																
TABLE I																	
False reproductions..	1	2	3	1	2	0	0	0	0	1	1	1	0	0	1	0	0
TABLE II Classification of reaction words in association experiment.																	
Misunderstood		1	1						1								1
Identity		1			1	1		1		1		1					
Perseveration																	
Repetition	1						3							1			
Word-Compounding				3		1										2	1
TABLE III Mistakes in cancellation test.																	
Omissions	0	1	0	2	3	2	1	1	2	3	2	0	2	1	0	1	1
Incorrectly crossed	1	0	0	1	0	0	0	0	0	2	1	0	1	0	0	1	0

Days	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34
														Fast ended			
TABLE I																	
False reproductions..	0	0	2	1	2	1	0	0	1	0	0	0	0	0	0	2	1
TABLE II Classification of reaction words in association experiment.																	
Misunderstood		1					1										
Identity																	
Perseveration			2	1					2	1	1	1			1	2	2
Repetition		1				1										1	
Word-Compounding		1		2					1		1				2		
TABLE III Mistakes in cancellation test.																	
Omissions	4	1	0	0	0	1	1	0	1	3	1	0	1	0	0	4	0
Incorrectly crossed	0	1	0	0	0	0	2	0	1	0	1	0	1	0	0	1	0

Later Tests	1	2	3	4	5	6
TABLE I						
False reproductions..	0	0	0	0	1	0
TABLE II Classification of reaction words in association experiment.						
Misunderstood						
Identity						
Perseveration		3	1			
Repetition				1	2	1
Word-Compounding	1 1		1		1	1
TABLE III Mistakes in cancellation test.						
Omissions	4	1	0		0	1
Incorrectly crossed	0	0	0		0	0

reproductions was very small (Table I), amounting to only twenty-three in 680 reactions or 3 per cent and never more than three in one list, an improvement or the reverse in this respect would mean little. At least one can say that the quality of the reproduction suffered no deterioration with the progress of the fast, but that retention was equally as good at the end as at the beginning.

The quality of the association reactions was of high grade throughout the main test (Table II). There were no senseless or pure sound reactions and very few repetitions. Synonyms, word-compoundings and misunderstood stimulus words occurred seldom and were scattered throughout the days. The word woman appears a number of times and man slightly less often. There was also evidences of a religious complex.[17] An examination of the different categories did not show sufficient change to warrant an analysis or tabulation as to quality. It was thought that the introduction of words designating food might produce delayed reactions both with the word itself and with the words immediately following. This was not the case. For example on April 16th we find egg-white 1.4 sec.: on April 19th omelet-eat, 1.4 sec.: on April 21st fish-sea, 1.4 sec.: on May 7th candy-sweet, .8 sec.: on May 9th apple-fruit, .8 sec.: on May 10th, roast-meat, 1 sec.: on May 13th chocolate-sweet, 1 sec. None of these reactions were followed by unusually long reaction times. It might be of interest to mention at this point the unusually long reactions which point to complexes. On April 13th we find pulse-hand, 9 sec.: on April 21st death-eternal, 22.4 sec.: and on April 26th uncertainty-pendulum, 12.6 sec. These are the only extremely long reaction times. The next longest is 6 sec. All of these delayed reactions may be explained from the same cause. Levanzin had asserted that the chief factor for a successful fast was faith and confidence and absolute lack of fear. He thinks it is the fear combined with exposure which causes death in shipwrecks and other calamities where food is not obtainable and not the actual lack of food. We also find that those who fast frequently cover their mirrors in order that they

[17] See for example the list of April 30th.

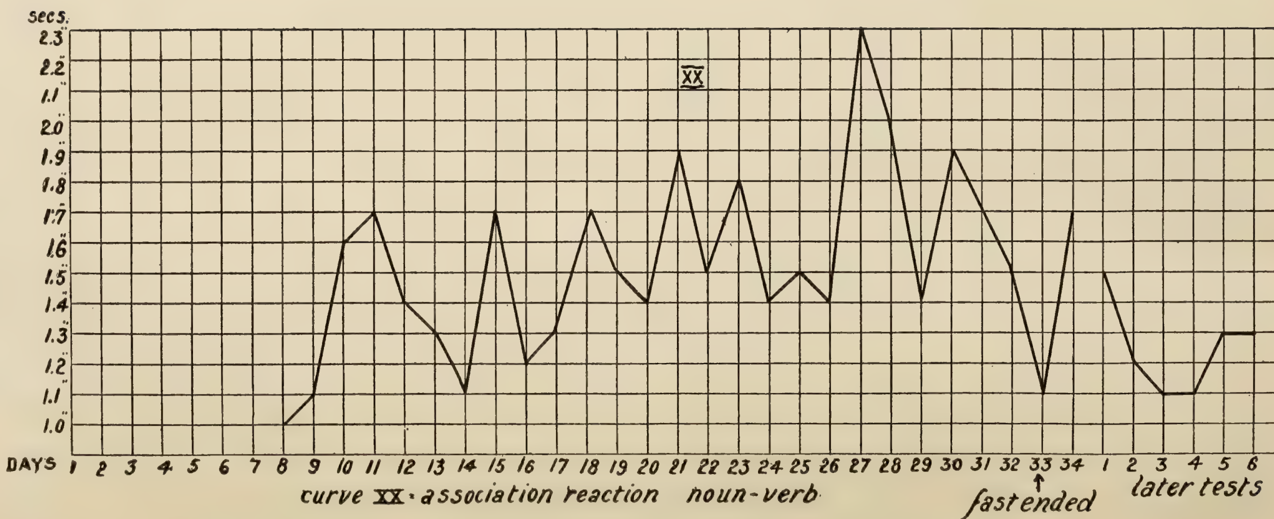
may not be disturbed by the evidences of emaciation. One of the further dangers in fasting is heart failure. If Levanzin's heart had shown alarming symptoms the fast would have been terminated at once. It does not, therefore, require a stretch of imagination to suppose that Levanzin would keep his mind from such subjects as death and uncertainty and that he would even avoid thought of the condition of his heart and that the mention of these words would cause hesitation.

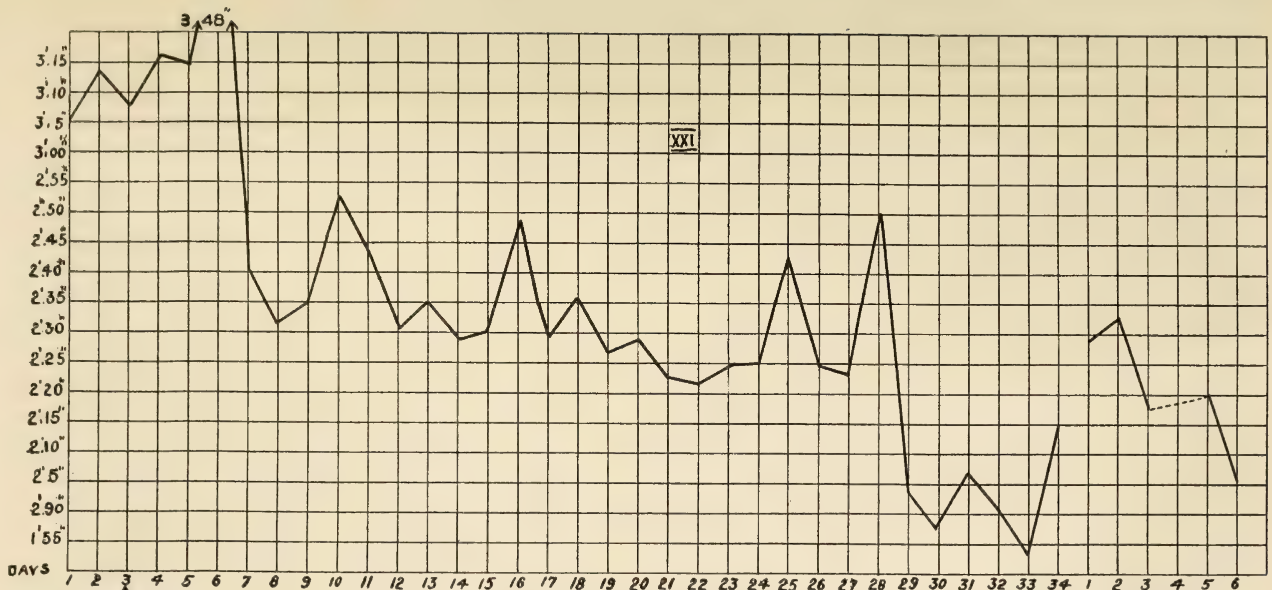
The determined association reaction noun-verb was begun on the 8th day. The curve (XX) resembles that of the verb curve, except that the rise continues longer. It starts very low,—1.0 sec.—increases with rather large daily fluctuations and on the last day of the fast returns to 1.1 sec. A particularly disturbing factor in this series was the fact that there was an ever increasing difficulty to obtain appropriate words. At first the words had obvious associations. They were names of common objects, such as dog, gun, eye, etc., but more unusual words had to be employed in increasing numbers and there seems no doubt that this circumstance was at least part cause of the increasing length of the reaction time. It is even more important in the determined than in the free association experiments to have the quality of the words the same and not more difficult. For long series of tests the free-association experiments are much to be preferred.

Cancellation Test

Special forms were made for this test consisting of type-written pied text of 100 a's and fifty of each of the other letters of the alphabet. A different combination was made each day so that the subject should not become accustomed to the order. Levanzin was requested to cancel all the a's. He used his left hand and the time was taken with a stop-watch. Special care was observed to have the illumination constant and the same pencil was employed.

The curve (XXI) represents the time for the completion of the task. As in some of the other curves so here we have the initial


secs.
2.3"
2.2"
1.1"
2.0"
1.9"
1.8"
1.7"
1.6"
1.5"
1.4"
1.3"
1.2"
1.1"
1.0"
XX
DAYS 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26 27 28 29 30 31 32 33 34 1 2 3 4 5 6
curve XX · association reaction noun-verb
fast ended
later tests



curve XXI = cancellation tests

rise, which continues to the 7th day, when there is a sudden drop to a level which slopes slightly to another sudden drop on the 29th day and a very low level for the final days. The difference between the maximum of 3 min. 48 sec. on the 6th day and the minimum of 1 min. 53 sec. on the 33rd day is very considerable. The maximum is over double the minimum and even if we compare the minimum with the initial time of 3 min. 7 sec. or with 2 min. 43 sec. of the 7th day which is the first and largest practice drop, we still find a very considerable difference. There does not seem any doubt therefore, that there is very much of a betterment in the time as the fast progresses and that this decrease in the time continues to the end of the series. Nor is this improvement in time gained at the sacrifice of accuracy. At no time were there many mistakes made (see Table III). In fact the degree of accuracy was always so high that we cannot place any importance on the slight increase of accuracy in the last half of the series[18] nor does the slight loss of accuracy at the minimum alter the significance of that result.

Visual Acuity

These tests were made in the large calorimeter room adjoining the small room in which the previous tests were conducted. The largest E which had been cut from the Schnellen test-card was used. This was held by the experimenter at the level of the subject's head when seated. It was illuminated by an electric lamp held by a second experimenter in front of the card and moved with it. The shades of the room were kept drawn during the experiment in order to have as far as possible constant illumination. The subject suffered from myopia and wore corrective glasses. A distance well within the threshold was first chosen at which the subject was asked to judge in what one of the four possible positions the E was being held. The experimenter put the card behind his back when he changed its position. After a few days of the tests it was thought that the subject might

[18] There were 29 mistakes in the first half and 24 in the second half of the series.

be using the secondary criterion of the distance of the edge of the E from the edge of the card, the E not being exactly in the middle. The card was therefore mounted on a larger cardboard of the same color in order to obviate this possibility. On account of the surprising results both experimenters were at all times keenly attentive to the possibility of other secondary criteria, but none could be discovered. Ten trials were made at each distance, the card being moved from the observer in steps of one foot. That distance was considered the threshold which preceded the distance at which the subject made two mistakes out of the ten trials.[19] The alteration in the position of the E followed no definite order, but every means was used in this respect to confuse the subject in order to remove all possibility of his guessing the position. Most of the judgments were made without hesitation, both at the very low and very high thresholds.

The curve (XII) represents the daily threshold in feet. There is a very rapid rise from the 5th to the 14th day when the maximum of 37 feet is reached. Then there follows a drop to 24 feet and a rise to 36 feet on the next to the last day of the fast. The 34th day shows a drop to 19 feet. The record of the 5th day is 16 feet which is the minimum; that of the 32nd day is 36 feet, which is one foot less than the maximum. This difference of 16 feet is very great for visual acuity. He sees twice as far at the end of the fast as he does at the beginning.

Later Tests

Owing to an attack of colic resulting from the nature of the first food taken after the fast and the subsequent withdrawal from the laboratory it was impossible to continue the tests during the recuperative period, as had been planned. Only by later tests for comparison could a decision be reached as to the efficacy of fasting. One year after the tests just described Levanzin volunteered as subject for a short series of tests. These were conducted at the Harvard Psychological Laboratory and ex-

[19] Time prevented the threshold being taken in the reverse direction. The tests took five to ten minutes.

tended over a period of six consecutive days. It was not possible to arrange for them to take place at five o'clock as previously and ten o'clock in the morning had to be chosen. All the other conditions were observed as closely as possibly. The same tests with the exception of the visual acuity test and the hand-writing test were performed. Levanzin seemed in good health. He weighed about 126 lbs. which is somewhat less than he weighed when he began his fast. His physical appearance was, however, very much the same as on the day he arrived at the Nutrition Laboratory. He had remained in America during the previous year, engaged in medical studies, lecturing, etc., had not fasted again and had had no illness during that time. In coming to the laboratory he made a journey of four miles and had already had several hours work, having risen each day at five o'clock, exercised for half an hour and made several visits. The conditions previous to the tests are, therefore, hardly comparable to those of the former series. It is evident, however, that he was as strenuous if not more so than he had been up to the later hour of five o'clock of the previous tests.

The rote memory for digits (IIA) was somewhat poorer than it was during the latter part of the long series. It did not reach the maximum by two numbers, yet it did not show any poor scores. The curve (IIB) which represents the first mistake, or, as it was supposed above, the state of attention, shows an improvement over the latter part of the first series in that it does not drop as far. On the other hand the rote memory for words (IA) seemed as good if not better than during the fast. It reached the former maximum on the fourth day and never dropped below eight words. The memory after fifty-five minutes (IB) was as good as the immediate memory. From these results it may be concluded that the memory is still, after the year's interval, at about the level that it was at the end of the fast.

The curve (III) for the tapping begins considerably higher than the maximum of the fasting tests and although it drops somewhat, still it remains above the former maximum. The drop in the difference curve (IV) is caused principally by a

falling off in the initial spurt. This is concluded from the fact that the results of the last ten seconds vary much less than those of the first ten.

The results of the first day of the tactual space threshold cannot be used as a comparison (XI). The unusually high threshold was undoubtedly caused by inattention on the part of Levanzin, who admitted that he had been very much worried over an appointment he had been forced to miss and upon which his mind had been during these tests. Apart from this day the curve has the same form it had during the latter part of the previous trials. The second and third days show the minimum, which was last reached on the 30th day of the former trials.

The dynamometer used in the previous tests could not be obtained until the second day. There are, therefore, only five records. The curves for both the right (VII) and the left hands (V) begin with very high records and drop considerably on the second day, just as they did in the former series. These first records are very much better than any made in the previous trials. Even after the drop the right hand twice surpasses the previous maximum and remains close to it on the other days. The difference curves (VI, VIII) show that on the first day the high record for the left hand is made by a sustained effort. The right hand spurt causes fatigue toward the end. The large differences during the next three days for the left hand are caused by spurts followed by fatigue, that of the right hand by fatigue. It is seen that the strength of the muscles of the hand have very much increased since the end of the fast and judging from the first day's results is much greater than at the beginning of the fast. One acquires a knack in gripping the instrument and it may be that this is carried over from the former tests and makes these initial records higher than those of a year ago. In other words some of the effect of practice is still present and influences the results much more than it did when it had the opposing effect of fatigue.

The free association reaction time (XIII) begins at the low point of the last day of the previous series, on the third day it

reaches the shortest time of that series and again on the fifth day and on the last day it falls almost one-fifth of a second below this point. That is the curve continues the descent it began in the middle of the former series in as regular a manner as if a year had not intervened. Inasmuch as some practice is necessary after so long an intermission, it may be said that the reaction times are better than they were at the end of the fast. The m.v. (XVIII) was .5 sec. on the first day and .15 sec. on the sixth with an almost steady decline.

The average reproduction time (XIX) is .9 sec. for all the days. This is very low and although .8 sec. was reached three times in the former tests, it is safe to conclude that the reproduction times are at least as good as they were at the end of the fast. In fact the average for these days is better than for any six consecutive days of the previous tests. There was only one false reproduction and that was "wrong" for "bad." In view of the fewness of the trials little would be gained by an analysis of the results according to categories (XIV, XV, XVI, XVII). The noun and adjective curves are lower than the verb and abstract curves. The quality of the reactions is about the same. Evidence of a religious or mystic complex is as plain here as in the previous results. "God" was the reaction for "adore," "worship," "unseen," "mercy," "Divine," and "Infinite;" "supreme" gave "Being," "sacred" gave "church," "adorable" gave "saint," "life" gave "eternal," and "ornament" gave "church." There were no very long reaction times. In connection with the previous complex it may be mentioned that death was the reaction word for fear.[20]

The reaction noun-verb (XX) begins at the average of the 32d day of the former series and on the third and fourth day reaches the minimum of the next to the last day of the long series. The average of these days is very much better than that

[20] It was thought that a year's intermission would make the old lists equivalent to new ones and as one would then be sure of having the lists of this series of the same quality with those of the former, the old lists were used on the first day, but seven of the twenty reactions were the same as those made a year ago, so that new lists were made.

of the last days of the fast series or even of the first days so that there is no doubt of an improvement in these reactions.

The cancellation test (XX) begins at about the point of the 27th day and the time gradually decreases, but at the sixth day has not reached the rapid time of the 33rd day. Judging from the slope of the curve one would expect it to do so shortly, however, so that one can conclude that the mental functions necessary for this test, are in about the same state they were at the end of the fast. There were only six mistakes, four of them being on the first day.

Correlations

It would be supposed that there should be very good and very poor days upon which all the curves would show proportionate increases or decreases or that at least similar tests, such as those of the higher mental-processes, would show similar variations. If we compare some of the crests and valleys however, we arrive at negative results. For instance, on the 22d day the association time (XIII) is long and both memory curves (IA, IIA) are in a valley, but the cancellation test (XXI) shows improvement, and the reproduction times (XIX) are not long. On the 16th day the left hand reaches a maximum in the strength tests (V), but the right hand (VII) shows no such result. Tapping (III) rises on that day, but it is still comparatively low, one memory curve has fallen (IIA) and the association time (XIII) has risen. On the 15th and 17th days the memory curve (IIA) is at a maximum and association time (XIII) is also lower, the cancellation test (XXI) is also low on these days, but the maximum of the memory tests (IA, IIA) on the 31st day finds the association times (XIII) longer. On the 12th day the curves for the strength tests (V, VII) have risen for both hands—it is the maximum for the right hand—the time for the cancellation tests (XXI) has shortened and memory (IA) is better, but the tapping record (III) has fallen and both association (XIII) and reproduction times (XIX) are at a peak. The considerable lengthening of the time of the cancellation

test (XXI) on the sixth day finds a betterment in most of the other tests, the tapping test (III) indeed, having reached its maximum on that day. The visual acuity curve (XII) rises abruptly to its maximum on the 14th day and although with a few exceptions the curves show a slight betterment, the rise is comparatively insignificant.

It must be concluded, therefore, that with the exception of this last day the daily fluctuations cannot be traced to any one cause such as a general bodily fatigue and depressed mood or vigorous and cheerful mental states, but that either there is a change in the one or more processes essential to the particular test that is showing the exceptional rise or fall or that there has been a momentary wave of fatigue or distraction or spurt, etc. A diary of the fast was kept in which every important incident was noted and it is possible that many of the fluctuations in particular curves or changes in general tendencies of several of the curves could be more or less satisfactorily explained. The following considerations, however, make such explanations of doubtful value. One cannot say in advance what the effect of visits or other changes in the general routine may be. Much depends upon the particular circumstances. Now if the results were better after a certain visit one could say that the subject was in a pleasant mood after the break in the monotony of the days and that his mind had been stimulated by agreeable conversation. If the results were worse on those days one could say with equal weight that the fatigue following the unusual exertion was the cause. Only the most reliable introspection on the part of the subject before and after each test could have given strength to such explanations and both the lack of time and training on the part of the subject made such a procedure impossible.

It did seem possible, however, to make an exception of the days that Levanzin took a drive or was allowed on the roof and that if the curves showed an agreement in their fluctuations on these days an unequivocal explanation could be found. The drives were taken on the 14th, 17th, 20th, 22nd, 24th, 29th,

31st and 32nd days; the visits to the roof on the 11th, 15th, 21st and 30th days. As was stated above there was no general agreement even on these days. In regard to the individual curves, however, the visual acuity curve seemed to show the influence of the drives. The best result in the visual acuity test was made on the first drive day and the curve always ascends on the drive days, although not always to a peak. It falls, however, on all but one day when a visit was made to the roof and that it rises on the drive days is contrary to what one would expect and is difficult of explanation, since the subject's eyes should if anything have been fatigued by the increased light. If there had been a stimulation of the central processes causing a heightened power of discrimination, this ought to have influenced the other curves as well.

General Summary and Conclusions

The fact that a human being could live for a month or longer without food had already been satisfactorily proven.[21] In recent years Merlatti is reported to have fasted for fifty days and Dr. Tanner for forty days. The fast of Succi[22] is most similar to that of Levanzin in that it was undergone for about the same length of time and under similarly strict scientific control, although never before had quite so many precautions been taken as in the case of Levanzin. Succi fasted for thirty days, but took pepton on the 27th day. Levanzin continued for one day

[21] E. Bardier in his article "La Faim" (Ch. Richet's Dictionnaire de Physiologie. Vol. 6, p. 3) remarks in regard to voluntary and involuntary fasts: "on pourra se soumettre volontairement à un jeûne prolongé, comme l'expérience en a plusieurs fois été tentée, et endurer assez facilement les souffrances de la faim. Le besoin de manger sera d'autant moins douloureux, d'autant plus facile à supporter qu'il suffira d'un signe pour être mis en face d'un succulent repas. Au contraire, la faim sera beaucoup plus pénible ses manifestations beaucoup plus douloureuses, si l'on se croit-dans un naufrage, dans une expédition,—voué à une inanition complète sans espoir de salut." On page 6 in reference to forced fasting he further says: " . . . la lutte que l'on est obligé de soutenir contre les causes mêmes de cette inanition augmente la sensation de faim."

[22] Das Hungern, by Luigi Luciani. Translated into German by Dr. M. O. Fraenkel. 1890.

longer, absolutely nothing but 750 cc of distilled water passing his lips during that time. Both men remained in good physical condition throughout and seemed at no time to suffer any unusual discomfort. It was with difficulty that Levanzin was persuaded to discontinue his fast on the 31st day. Although Luciani doubted that Succi was mentally normal, general observations and the tests pointed to a sound mind in the case of Levanzin. Both men were, naturally, men of great determination and above all of implicit faith and confidence in their idea. Levanzin believed fasting to be a panacea for all ills and the very fact that he is of that type of man who can narrow his horizon about an idea and stubbornly resist all invasions, gave him the best equipment for the fight against the natural demands of the flesh. Such a type of mind cannot be called abnormal, although it is unusual. The feeling of hunger was at all times even during the first stages of the fast denied by Levanzin. This statement should not be disbelieved even though the general experience of most men is extreme discomfort, which those who fast tell us only disappears after the second or third day as in the case of Succi. With Levanzin and perhaps with other fasters this feeling of hunger may have been suppressed from the beginning by autosuggestion. The fact of the deep ingrained faith in the fast makes this plausible.[23]

The condition of Succi's higher mental processes was only ascertained by general observation. These agree with those upon Levanzin. There was at no time any symptom of hallucination or lack of clearness in the thought processes. Luciani writes:

[23] E. Bardier, in criticising Bernheim, writes: "Au sens où l'entend Bernheim, let jeûneurs qui se soumettent à l'inanition résistent facilement, tout simplement par le faia d'une auto-suggestion. Discutant en particulier le jeûne de Cetti, il admet que ce dernier-tout en n'étant pas un hysterique—s'est suggestionné. Il demeure convaincu qu'il conservait toute sa force physique, 'cela suffit pour réaliser le phénomène; l'idée fait l'acte; il s'exalte, il s'entraîne, il se nourrit de son idée, il se montre avec complaisance à ses visiteurs, il jouit de son triomphe; l'esprit domine le corps; etc.'...... Le jeûneur, par sa volonté, arrive à résister a l'habitude de manger; il obéit à sa conscience qui le somet a l'abstinence, mais certainement sa volonté doit être incapable de provoquer la suppression d'une sensation." Op. cit. p. 10. See also footnote p. 3.

"Am 13 Hungertage wollte ich seine Ausdauer bez. geistiger Anstregungen auf die Probe stellen, indem ich ihm schwierige oder unlösliche metaphysische und theosophische Fragen vorlegte und beständig Einwürfe gegen seine Antworten erhob, in der Absicht, seinen Verstand zu ermüden. Ich muss gestehen, nicht bemerkt zu haben, dass sein Geist dabei mehr ermüdete als der jedes andern Sterblichen von gleichem Bildungsgrade und gleicher Begabung, wenn man ihn solchergestalt martert."[24] Levanzin is a man of a much higher level of intelligence and intellectual training than Succi. At all times during the fast he was very eager to enter into discussions upon abstract subjects such as the value of the Esperanto language, the political conditions in Malta, the possibility of mental telepathy and theories of spiritism as well as the value of fasting. It could not be observed that there was any diminution of his argumentative powers or lack of lucidity of expression. When aroused to counter argumentation he showed the same energy in reply at the end as at the beginning of the fast.

Succi's muscular strength as well as his sensory acuity was ascertained in a manner somewhat similar to the method employed for Levanzin and the results will be compared in the following summary and interpretation of results:

1. In the Dynamometer tests made upon Succi it is impossible to tell from the text how many trials were made daily. As the curves for the ten trials and for the initial trial for Levanzin are similar, the ten trial curve will be considered. It is safe to assume from lack of mention of the fact and from the nature of the curves that Succi was right handed. It will therefore be necessary to compare the curve of the right hand of Succi with that of the left hand of Levanzin.

It will be remembered that the strength of both hands was found to increase after the drop on the second day until the right hand (VII) reached its maximum on the 12th day and the left hand (V) on the 16th day, both curves then dropping steadily from this point, the right, however, less than the left,

[24] Op. Cit. Pp. 68-69.

for the left reached a minimum on the 31st day, while the right during the fast never dropped as low as the record of the 19th day. There is a very striking similarity between them and Succi's tests.[25] Both of Succi's curves also drop after the first trials and then rise again, his left reaching a maximum on the 14th, his right on the 20th day, as compared to the 12th and 16th days of Levanzin. Succi's curves then drop also, but the left drops more than the right which is the reverse of Levanzin's curves. With Succi both maximums are greater than the first day's records while with Levanzin this is only the case with the left hand. This agrees, however, with Levanzin's records for the initial daily trials (IX, X). Further Levanzin was able to make a spurt at the end of the fast with both hands, this spurt extending through several days. Succi was only able to spurt with one hand and that on the last day, the curve for the other hand remaining stationary.

Luciani attributed the rise of the curve alone to autosuggestion. It seems quite probable, inasmuch as both men believed that their strength would be increased by the fast, that this idea strengthened their determination and that they bettered their results by sheer "will power."[26] There is, however, another possibility which may be assumed without denying the influence of autosuggestion and that is that at least in the case of Levanzin, who was unused to such tests, the coördination of the muscles became gradually more perfect and further that these muscles, which were being exercised daily increased for a time in strength as they would have done under normal conditions, but in this case possibly to the detriment of other muscle groups. In both cases with both hands fatigue gained the ascendency over practice effect and possibly over autosuggestion about the middle of the fast, causing the curves to drop. In the case of Levanzin's unpracticed hand, however, the effect of

[25] Op. cit. p. 55.

[26] E. K. Strong, Jr., in his paper entitled "The Effect of Various Types of Suggestion upon Muscular Activity" (Psych. Rev. 1910 Pp. 278) says: "The auto-suggestion tends most strongly of all the types of suggestion to heighten the maxima."

practice had more room to work and held the curve up longer than in the case of the practiced hand.

2. The tapping test (III) is also influenced by the condition of the muscular tissue, but there is another factor more essential here than strength and that is the reaction time. As in the strength tests so here there is a rise at first, but here it is of much shorter duration, the maximum of 215 taps in 30 seconds being reached already on the sixth day. The following considerable drop until the 15th day, at a time when the strength tests are showing more efficiency, may possibly be caused by a lessening in the interest for this test.[27] About the middle of the series this interest and increased effort for a good record may have returned, judging from the results, but fatigue had by that time set in and the curve, although rising until the last day is never quite able to reach the maximum of the sixth day; that is, there was some falling off in the rapidity of reaction, which judging from the results of the strength test is due rather to a change in the muscle tissue than to a change in the nervous arc.[28] From what we know of the effect of practice in such tests it is most probable that if it had not been for this increased muscular fatigue the curve would have reached an appreciable maximum at the end of the series. From the fact of the very

[27] See pp. 7 and 9.

[28] As the tapping tests preceded the strength tests the objection can not be raised that the hand was being unusually fatigued by these latter tests.

In reference to the tapping test under normal conditions Well writes that "The objective fatigue phenomena which we note in the test are in all probability fatigue phenomena in the refractory phase or a lowered efficiency of coördination, equally a product of altered synaptic conditions; the sensations of fatigue on the other hand, may with squal assurance be ascribed to tissue changes within the muscles that take place as a result of their continued effort." (F. L. Wells. Normal Performance in the Tapping Test before and during Practice, with Special Reference to Fatigue Phenomena. A. J. of Psych. 1908, p. 473.) In the above tests the change in muscular tissue is due to emaciation, a fact that does not play a rôle in the test to which Wells refers. At no time did Levanzin speak of sensations of fatigue and judging alone from his facial and bodily expressions there is no data from which to assume that they were greater at the end than at the beginning of the fast. As to the synaptic conditions there is nothing in the test to point to a change.

small difference between the average of the first ten and last ten seconds on the sixth day when the maximum was reached, as compared with the great difference in the almost equally good result of the last day, it is evident that on the first day the good performance of the first ten seconds practically continues throughout (in both instances the best record was made during the first ten seconds) while on the last day the effect of practice as shown in the initial performance was counterbalanced toward the end by fatigue.[29] These results seem to cast further doubt upon Luciani's hypothesis of autosuggestion in the strength test, for surely autosuggestion should play as great, if not a greater, rôle in the tapping tests during those days in which according to the strength tests it would have to be assumed at work. The results of the tapping test are indeed directly opposed to such a theory.

To sum up it may then be said that although initial lack of interest and later muscular fatigue played a rôle, both factors being directed toward a decrease in the amount of work, yet the nervous impulse toward the end was sufficiently great to bring the curve back to its initial level and almost to its maximum.

3. The threshold for tactual space perception (XI) decreased somewhat as the fast progressed. It was on the average much better during the last half than the first half of the series. Similar tests were made upon Succi upon a number of different parts of the body, but only on three days, before the fast, on the 15th day and on the 29th day. On some parts of the body there was an increase on other parts a decrease. Luciani believed the difference in the three days due to differences in degree of attention. On that part of the body corresponding most closely to the spot used in these tests i.e. the lower third of the volar side of the forearm, there happened to be a rather large decrease in the threshold, the three thresholds being respectively 16, 11 and 10 mm.[30] Authorities differ as to whether practice lowers

[29] Wells writes "the true practice gain is one mainly in the initial efficiency of performance, as distinguished from the warming up gain, which shows itself chiefly in continued efficiency of performance." Op. cit. p. 478.

[30] Op. cit. p. 64.

the threshold in tests performed under normal conditions. Dresslar[31] for example, found that practice had a considerable effect. Solomon[32] found that if the subject is not informed of his errors there is no effect of practice. In the above tests the subject was never told of his mistakes and "vexier" trials were introduced at frequent intervals and in no special order, yet there was a lowering of the threshold. This may and probably is due to several causes. A physiological cause would be a decrease in the fat thus exposing the nerve endings and making them more sensitive. On the psychological side increased attention, which we find indicated in other of the tests would lower the threshold for discrimination. Also as the tests progress the image of the criterion used becomes cleared. From what is known of the process of perception, this is a most important factor in explaining the above effect of practice. The physiological change is the only one which could be attributed unequivocally to the fast. The central change occurs in series under normal conditions.

If, as has been often assumed, the tactual space threshold test is a measure of mental fatigue, then it must be concluded that there is no indication of such fatigue during the fast.

4. The visual acuity (XII) showed an astonishing betterment. From 17 feet as the distance of clear vision for the particular test card employed, the curve ascended rapidly to 37 feet on the 14th day and, although there is a falling off, 36 feet is the record for the last day of the fast.

If it were not for the maximum of 37 feet midway in the series, the improvement would be comparatively a steady one. One explanation that suggests itself is that the possible change in intra-occular tension caused the eye-ball to change its shape. Unless his glasses were not the proper ones for him, however, a change in the eye should cause more rather than less difficulty as long as he wore his glasses. Further the suddenness of the

[31] F. B. Dresslar. Studies in the Psychology of Touch. A. J. Psych, pp. 313-368. 1894.

[32] L. M. Solomons. Discrimination in Cutaneous Sensations. Psych. Rev. p. 246-250. 1897.

rise seems to vitiate such a theory. A satisfactory explanation seems difficult to find. It might be said that the 37 feet record was made by chance. This also seems precluded by the fact of the number of previous steps in which 10 correct answers were given and from the evidence of confidence displayed by the subject.[33]

Succi's eyes were examined with the opthalmoscope and his acuity measure before the fast and on the 15th and 28th day of the fast, but no change was detected.[34] If Levanzin had happened to be measured on the third, sixteenth and one of the days toward the end of the series only, the change would have been thought as negligible as in the case of Succi. In all such tests where the daily fluctuation is considerable three tests in a month are not sufficient upon which to base a judgment as to the change in sensory acuity or higher mental processes.

5. The rote memory for digits (II) showed very little change. There is a slight suggestion of improvement during the first half of the series. Judging from the curve which indicates the point at which the first mistake was made (IIB), one can say that there was a gradual improvement in this respect, especially in the first half of the series, which is probably in part due to a betterment in the perception of the spoken word, but especially to an increase in attention, it becoming more sustained as the fast progressed. The rote memory for sense words (IA) showed a greater improvement than did that for digits. Here probably the practice effect consisted in the forming of associations between the words. The most marked improvement of all is in the retention after a longer period of time, i.e. after 55 min. (IB). This is probably also due in part at least to the more frequent forming of associations. In addition the repeti-

[33] The subject did not know whether he was right or wrong or how many correct answers constituted a threshold, so that the results could not have been prearranged by him. And if they could have been he would not have allowed such a good record already on the 14th day. The high threshold on the last day is obviously due to his unusually poor physical condition (when if at any time one might be justified in speaking of a lack of effort).

[34] Op. cit. pp. 66-67.

tion of the same task through so many days undoubtedly strengthened the determining tendency, i.e. the determination taken at the time of memorizing for the words to appear in consciousness again, it remaining either in consciousness or subconsciousness during the interval. According to Levanzin's statement his mind did not revert to the task within the hour.

Experiments upon memory under normal conditions also show the effect of practice as evidenced by an appreciable increase in the memory span which may continue for a period of two months.[35]

6. The Cancellation test, (XXI) which employs to a greater degree the higher functions of perception and attention shows the greatest improvement of any of the tests used. This improvement continues from the sixth to the last day of the fast. The accuracy is so high throughout the series that the slight improvement in the latter part of the tests is of no significance. Experiments have shown that fatigue affects the accuracy, so that again we have evidence against an increase in mental fatigue.[36]

Besides an improvement in the above named functions, the increase in visual acuity may have been a factor in the results. On the other hand, from the results of the tapping-test and strength tests one must conclude that the betterment is in no degree due either to a betterment in reaction time or motor ability.

7. The free association time (XIII) is on the whole shorter during the latter part of the series. If it were not for a rapid drop in the middle of the curve after a rise similar to that in the

[35] T. L. Bolton, The Growth of Memory in School Children. A. J. of Psych. 1892, pp. 362-380.

G. Müller & F. Schumann. Experimentelle Beiträge zur Untersuchung des Gedächtniss, Zeit. für Psych. 6. 1894, pp. 81-190, 257-339.

W. H. Winch. The Transfer of Improvement in Memory in School-Children. B. J. of Psych. 1908, pp. 284-293.

[36] B. Bourdon. Observations Comparatives sur la Reconnaisance, la Discrimination et l'Association. Rev. Phil. 1895, pp. 153-185. A. Binet. Attention et Adaptation, Année Psych. 1900, 6. Pp. 248-404. C. Ritter. Ermüdungsmessungen, Zsch. für Psych. 1900, pp. 401-444.

tapping-test the improvement would be comparatively steady. The minimum of 1.3 sec. is reached on the day before the last day of the fast and should be compared rather with the 1.9 sec. of the third day than with the 2.5 sec. of the first day, when Levanzin was unaccustomed to the manner of reaction. Even when this comparison is made it is seen that the improvement is considerable. A separation of the curve into four curves corresponding to the four categories used, made a more minute analysis possible. All the curves (XIV, XV, XVI, XVII) show fewer high averages in the second half of the series, but it is only in the abstract curve and in less degree in the adjective series that there are more low averages in the second half of the curve. In fact in neither of the other two curves is the lowest average of the first half of the series again equalled. This seems to indicate that the betterment in the general average of the twenty words is principally due to a betterment in the reaction to abstract words. It is to be expected that the most difficult associations would show the greatest practice effect. In the noun and verb curve there is an almost steady rise in the middle of the curve corresponding to the rise in the middle of the main curve. It seems plausible to suppose that there is here as in the tapping test a falling off of interest and that this would manifest itself more readily in the easier tasks, where the reaction is likely to become more nearly mechanical.

The general improvement is also seen in the decrease in the variations of the reaction times. In all four curves the daily variation is much less in the second half of the series. Parallel with this is the decrease in the variations within each day, as is shown by the decided drop in the m.v. curve (XVIII).[37]

Although the improvement in the reproduction time is not as great as in the association time yet it is noticeable, the average of the second half being lower than that of the first, although

[37] Wells conducted long series of association reactions with normal subjects and for all of them found an improvement in the reaction time. "Practice Effect in Free Association," Am. Jour. of Psych. 1911. Vol. 22, pp. 1-13.

the very low time of .8 sec. was made on the second day as well as during the second half of the series.

The quality of the associations was good throughout, (Table II) and showed no striking change.[38] The reproductions were so nearly perfect from the first that nothing can be said in regard to them to support the results of the memory tests. One might add, however, that neither do they contradict those results.

The controlled reaction noun-verb (XX) shows an increasing lengthening of the time until almost the end of the series. It is quite probable that this was caused by an increasing difficulty in the stimulus words selected, a factor which could not well be avoided. No other reason suggests itself why these reactions should have taken a different course from that of the free association tests.

The present methods of testing mental capacity unfortunately do not permit one to make dogmatic statements as to the results of any such tests. In each one a number of functions are involved any one of which may have produced the variations which occur. For example in the cancellation test there is involved among other things attention and interest, apperception and discrimination, nervous impulse and motor discharge. When, however, as here, a set of tests are employed in which the same

[38] W. Weygandt's results are hardly comparable to those obtained in these tests (Ueber die Beinflussung geistiger Leistungen durch Hunger, Psych. Arbeiten, 4, pp. 45-173). His subjects fasted for periods of only twenty-four and forty-eight hours at a time. This intermittent fasting seems to cause a much more pronounced disturbance to the organism than a prolonged fast. That there was greater exhaustion seems to be indicated by the fact that there was an increase in the associations by sound. He also finds that there was an increase in the outer as compared with the inner associations. (It is now admitted that such a classification of reaction words cannot be made without introspection.) Weygandt also found memory to be effected. The association time was not altered. Aschaffenberg studied the effect on association reactions of the exhaustion produced by a night's work without food or sleep. (Studien ueber Associationen, II Teil. Die Associationen in der Erschöpfung. Psych. Arbeiten, 2, pp. 1-83.) He too found a similar decrease in the quality of the reaction words. "Mit der Zunahme der Erschöpfung wirkt die zugerufene Vorstellung immer weniger durch ihre Inhalt; an dessen Stelle bestimmen der Klang und die Tonfarbe die Reaction."

functions are more or less active and they all show a similar trend, then a conjecture along general lines seems legitimate. And further when there is a very decided difference and it is known that a certain function is of prime importance then one is undoubtedly justified in ascribing the outcome of this test to changes in this function. It is desired to make it plain that no exact measurement is claimed, but merely that it has been possible by means of a number of selected tests to sketch an outline picture of the condition of Levanzin's psychophysiological organism.

It will be remembered that the tests range from those involving principally the muscle groups to those depending in a higher degree upon central factors. The tests depending most on the muscular reactions i.e. the strength test, showed a falling off. The tapping test which also involved the muscles but in which the rapidity of reaction was a more important factor showed no improvement. As soon as one turns, however, to the sensory discriminations one notices an increased efficiency, which is probably due either to a change in the peripheral organs, or central processes or both. Finally all the tests involving the higher processes of attention, perception and association show improvement. *In a word there was a loss in muscular strength due probably to loss of tissue, a possible gain in sensory acuity and a decided increase in the efficiency of all the central processes.* It would be premature to say that the improvement is the direct result of the prolonged abstinence from food, in as much as similar improvement has been observed in such tests under normal conditions due entirely to the effect of practice. *It can be stated, however, with some degree of certainty that the complete abstinence from food for thirty-one days had little effect upon the higher mental functions which were able to develop through practice very much as they would have done under normal conditions.* This agrees with the observations upon the physiological conditions. It has been found that during a fast the muscle tissues are the first to suffer and the nervous tissues the last. From these results it seems that up to the thirty-first day the nervous tissues have not suffered.

These results also confirm in part the general observations made by those fasting. It is frequently stated by them that they can do better mental work. The results show that at least they can do approximately as well, and it is not at all unlikely that some can do better, for it must be remembered that there is none of that sluggishness of the mental processes directly after eating, when the digestive processes are at their height and there is also absence of indigestion and the after effects of alcohol, caffeine and tobacco. That, on the other hand as has been often claimed, they are able to do more muscular work and that their power of endurance is greater is in this case at least not true. Probably the contrast of their actual results compared with what they expected would happen to a man without food makes the result seem greater than it is. The claim that the senses are more acute has been verified as to the visual acuity. It is hardly likely that the slight difference in the tactile space threshold would have been noticed by the faster.[39]

The question remains as to whether prolonged fasting is beneficial or dangerous to the organism. This can only be satisfactorily answered after an exhaustive physiological examination extending over a long period of time subsequent to the fast. The tests made after the lapse of a year permit, however, of some conjecture in this regard concerning those functions at least which have been discussed in this paper.

The strength test shows a great improvement over the former record. Levanzin exerted a pressure considerably greater than at any time during the long series. The record for the tapping test is also above the maximum of the previous record. The association test shows a marked improvement and the reproduc-

[39] Levanzin stated that the heightened sensitivity for odors made walking on the streets of Malta during his first fast positively unpleasant.

The other senses were examined in the case of Succi and no appreciable change discovered. Op. cit.

Whipple, op. cit. p. 215, in speaking of the effect of practice in the aesthesiometer test remarks that Dresslar states "this practice effect is . . . rapidly lost, being reduced very definitely within eight days and completely lost within a month.

tion is also better, especially in that it varies less, and the retention of sense words has perhaps also slightly improved. The tactual space threshold and the rote memory for sense words are about the same as at the end of the fast. Only in the case of the memory for digits and in the cancellation test has the previous maximum not been reached, but both of these results show consistently good results. It may be stated in short that after an entire year's intermission the curves continued practically from the point they had previously reached if not considerably above that point without showing that loss of practice which might well have been expected. These improved conditions are, however, not necessarily traceable directly to the beneficial effects of the fast. In regard to the association tests Levanzin has undoubtedly become still better acquainted with the English language and in respect to the strength tests it must be noted that Levanzin has exercised his muscles daily according to his report. In general he has lead a careful life paying especial attention to his diet. There is also the possible effect of climate and his new surroundings with which to reckon. Finally and most important is the possibility that there was actually a greater effect of practice in the first series than appeared in the records but that it was concealed by certain opposing effects of the fast, so that the results of the later tests may not be quite what might be supposed from a comparison of the records.

It remains, however, an undisputable fact that, according to the tests made there was no lasting evil effect of the fast, either upon muscular strength or mental activity and that from one cause or another Levanzin was, if not in better condition, certainly in as good health after as before the fast.

The author takes this opportunity to express his appreciation of the kind assistance of Dr. F. G. Benedict, Director of the Nutrition Laboratory and of the advice in the selection of the tests given by Professor Raymond Dodge. Thanks are also due to Mr. Carpenter and the other members of the Laboratory for their ever ready aid.

Appendix I

Dreams

As has been already stated Levanzin was asked to recount the dreams he had had during the previous night. From these records those dreams are here given which pertain to food. It will be seen that at one time he ate, at another refused food, but in neither case was there evidence of anything but a normal emotional reaction. According to the Freudian theory this absence of an intense emotional state (there were no nightmares nor anything else in the records indicative either of mental or bodily distress) means that the will ("wish") to fast was too strong to allow of any serious conflict of ideas. A great part of the dreams are of a sexual nature and are not here given.

April 13th. I saw a basket covered with a white piece of cloth, which I imagined full of food. When I tried to uncover it several black rats jumped out of it and frightened me.

I dreamed I was passing down one of our streets in Malta with a paper bag under my arm containing cheese cakes for my daughter. I found myself in a state of mental excitement and after going a certain distance I found that the lower end of the bag was opened and the cheese cakes were gone. In their stead was a white hand.

April 19th. I dreamed I was in a shop and on the counter there was a very big ham of about ten feet diameter. The proprietor was riding on the top of it with a knife in one hand. "It is a very good one," he said. I answered, "I do not like it. Do you not know I am fasting?" Then a Friar came in and said, "I will take it in his stead, because I like it." He took it and swallowed it.

April 21st. I dreamed I had been for a walk in the country. I went to a country tavern and asked for something to eat. He gave me a beefsteak and some fried red fish. I ate them with relish and asked what I had to pay. He told me $1.50 and asked if that was too much. I said I did not think so. In coming out of the tavern I saw a river full of these red fish and people were trying to catch them. I said, "You are fishing out all the fish and if you continue you will not have any more to eat."

APPENDIX II.

April 11, 1912

Stimulus Word	Reaction Word	Reaction Time
Paper	ink	2.2
Bright	light	2.
Yellow	lemon	1.8
Table	knife	1.2
Spoon	broth	2.8
Apple	stem	2.4
Sleep	bed	1.6
Room	door	1.3
Face	eye	2.0
Carpet	red	1.8
Animal	white	2.6
Rain	noise	5.
Teach	bench	2.
Doctor	knife	4.
Book	no. of pages	3.8
Store	glass window	1.6
Horse	tail	2.2
Island	trees	2.2
Journey	ship	2.2
Freedom	banner	2.0
Sweet	sugar	1.2
	Average	2.3

April 13

Stimulus Word	Reaction Word	Reaction Time
Timid	rabbit	3.0
Pulse	hand	9.0
Mystery	religion	5.2
Savage	wolf	2.4
Spirit	angel	2.4
Teeth	to eat	2.6
Bargain	profit	5.0
Blunder	mistake	3.0
Temper	nervous	2.2
Abrupt	cascade	2.0
Harp	sing	2.0
Switch	machine	2.4
Wide	sea	2.2
Tailor	stuff	3.0
Income	money	1.5
Splendor	sun	1.8
(Salve) Solve	lip	2.5
Moon	silver	2.2
Frost	white	1.8
License	wine	1.8
	Average	2.9

April 12

Stimulus Word	Reaction Word	Reaction Time
Round	table	2.0
Country	green	1.8
Silver	spoon	2.3
Rabbit	white	2.0
Chair	cushion	3.0
Glass	window	2.0
Flower	odor	2.3
Sun	brightness	3.2
Bread	white	2.3
Wood	hard	3.0
Well	water	2.4
Danger	sea	2.0
Tired	bed	2.0
Watch	gold	2.4
Marble	table	1.6
Iron	bar	3.8
Bridge	iron	2.8
Blind	dark	2.4
Pencil	wood	3.0
Candy	sweet	3.4
	Average	2.5

April 14

Stimulus Word	Reaction Word	Reaction Time
Accept	a reward	2.8
Air	blue	2.6
Able	sailor	2.0
Abuse	drink	2.6
Address	letter	1.8
Blood	red	1.1
Bad	man	1.4
Age	90	1.2
Agree	wife	1.0
Boot	black	1.8
(Tall) Ball	tree	1.6
Balance	weight	1.6
Amuse	theatre	1.4
Bottle	ink	1.4
Band	brass	1.4
Climate	mild	.8
Bite	dog	1.5
Box	wooden	1.6
Contents	book	6.4
Boy	small	1.8
	Average	1.9

April 15

Stimulus Word	Reaction Word	Reaction Time
Catch	bird	1.6
Brain	human	2.6
Broad	street	1.4
Courage	man	2.2
Cease	speak	2.2
Brick	red	1.6
Broken	glass	1.0
Culture	physical culture	1.6
Compel	servant	3.4
Cable	iron wire	1.4
Central	station	1.6
Crowd	people	1.2
Confess	priest	1.0
Carbon	carbon dioxide	1.8
Common	sense	1.0
Day	night	2.0
Control	engine	1.0
Chain	iron	1.0
Course	study	2.2
Delegate	apostolic	2.0
	Average	1.7

April 17

Stimulus Word	Reaction Word	Reaction Time
Crawl	serpent	2.0
Clown	buffoon	2.4
Dizzy	headache	1.6
Distance	my country	2.0
Cure	physic	2.6
Corn	grass	1.8
Easy	chair	1.8
Distress	sorrow	2.0
Decorate	church	1.6
Copper	metal	1.4
Even	ground	2.4
Endurance	fasting	1.4
Decline	age	1.0
Cream	sweet	2.0
Firm	strong	3.4
East	west	1.0
Degrade	man	1.8
Corset	woman	1.0
Flat	floor	1.8
End	book	3.0
	Average	1.9

April 16

Stimulus Word	Reaction Word	Reaction Time
Defend	Country	1.8
Deck	ship	1.2
Fresh	air	.8
Faculty	arts	1.0
Deduct	sum	1.4
Dinner	good	1.4
Flavor	odor	2.2
Displease	anyone	3.2
Dog	large	2.0
Good	man	.6
Fault	his fault	3.0
Egg	white	1.4
Green	tree	1.8
Fright	dog	2.2
Drive	horse	1.2
Fairy	tale	1.4
Hard	stone	.8
Function	ceremony	3.2
Profess	religion	1.4
Salt	sea	1.4
	Average	1.7

April 18

Stimulus Word	Reaction Word	Reaction Time
Hit	hammer	3.0
Swallow	food	1.2
Suffer	pain	1.2
Build	house	1.3
Rubber	teeth	1.4
Food	good	1.0
Park	large	1.1
Boat	swim	1.8
Smooth	floor	1.1
Straight	way	1.8
Ugly	man	1.8
Gentle	woman	1.4
Naughty	man	2.0
Power	England	1.6
Strength	athlete	1.9
Charm	woman	3.0
Cost	money	1.0
Kindness	woman	2.2
Break	glass	1.2
Jaw	mouth	1.8
	Average	1.6

April 19

Stimulus Word	Reaction Word	Reaction Time
Produce	field	1.4
Cry	baby	1.0
Freeze	cold	1.6
Follow	soldier	5.8
Smoke	pipe	.8
Rope	long	2.0
Omlet	eat	1.4
Cap	head	1.0
Burglar	thief	1.6
Delicate	woman	.8
Thick	paper	2.8
Expensive	money	1.0
Dark	night	1.0
Unfair	unjust	2.0
Purpose	scope	1.0
Glory	eternal	1.2
Mischief	bad	2.0
Occasion	accident	1.0
Nuisance	wrong	1.6
Overcoat	dress	1.0
	Average	1.6

April 20

Stimulus Word	Reaction Word	Reaction Time
Prefer	office	2.4
Crush	crowd	2.0
Allow	pension	1.6
Drink	water	1.2
(Solution) Salute	salt	2.2
Hip	thigh	1.2
Lightening	thunder	2.0
Parlor	bedroom	2.4
Snake	serpent	1.0
Wicked	man	1.2
Rich	millionaire	1.8
Clean	body	1.2
Bashful	woman	1.0
True	religion	5.2
Exchange	money	1.0
Style	literature	1.0
Power	gun	1.0
Result	good	1.4
Nonsense	foolish	1.6
Seed	plant	1.0
	Average	1.7

April 21

Stimulus Word	Reaction Word	Reaction Time
Pinch	pin	1.4
Satisfy	appetite	.8
Nourish	food	1.2
Drift	wind	.8
Abuse	drink	1.2
Ditch	deep	1.2
Tiger	fierce	1.0
Music	sweet	1.0
Fish	sea	1.4
Death	eternal	(22.4)
Soft	paste	2.4
Ugly	man	1.2
Watchful	policeman	2.6
Indecent	conduct	3.0
Haste	hurry	1.0
Comfort	good	2.0
Adventure	strange	1.2
Practice	long	1.8
Untrue	falsehood	1.6
Merit	high	2.8
	Average	1.6

April 22

Stimulus Word	Reaction Word	Reaction Time
Eat	bread	2.0
Open	door	1.0
Divide	reign	1.8
Fade	flower	1.6
Travel	ship	2.0
Umbrella	rain	.8
Gift	gold	3.0
Man	long	.8
Sailor	ship	1.2
School	teacher	1.2
Dense	air	2.0
Short	man	1.4
Weary	travel	1.6
Best	book	5.4
Excuse	pardon	1.6
Insult	bad	3.8
Prudence	woman	1.6
Caution	wise man	2.2
Conceit	ambition	2.2
Captain	ship	1.4
	Average	1.9

April 23

Stimulus Word	Reaction Word	Reaction Time
Collapse	sick	2.4
Excite	nervous	1.6
Begin	book	1.8
Prosper	progress	2.4
Hat	head	1.2
Sister	brother	1.0
Ham	meat	2.0
Crime	justice	2.8
Tight	shoe	2.0
Solid	stone	1.8
Cold	winter	1.6
Clear	sky	1.4
Hope	fortune	3.6
Dismay	fear	1.6
Offense	insult	1.4
Blunder	mistake	1.0
Future	time	4.0
Insist	persist	2.4
Trap	wolf	2.0
Oblong	square	1.4
	Average	2.0

April 24

Stimulus Word	Reaction Word	Reaction Time
Restore	furniture	1.4
Impress	printing	1.8
Flirt	woman	1.0
Ask	question	1.2
Receive	letter	.8
Baker	bread	1.0
Athlete	strength	1.0
Cradle	baby	1.0
Bundle	hay	1.0
Elephant	trunk	1.0
Cheap	money	3.0
Black	dog	.8
Tender	meat	1.4
Prompt	answer	1.4
Ignorant	man	1.0
Confidence	familiarity	2.0
Jealousy	woman	.8
Honesty	good	4.2
Unbelief	atheist	2.4
Heroism	warrior	2.0
	Average	1.5

April 25

Stimulus Word	Reaction Word	Reaction Time
Join	chain	1.8
Clasp	hand	1.0
Advance	pretention	2.0
Argue	discussion	2.0
Mountain	large	1.0
House	beautiful	1.4
Neck	strong	1.0
Lamb	quiet	1.2
Hero	brave	1.2
Jealous	woman	1.4
White	snow	2.0
Serious	man	1.0
Vacant	space	1.0
Fertile	land	1.0
Reason	mind	1.6
Protection	government	1.8
Solemnity	festivity	1.0
Impudence	woman	3.8
Convenience	etiquette	3.0
Scratch	nail	1.6
	Average	1.6

April 26

Stimulus Word	Reaction Word	Reaction Time
Forget	memory	1.2
Dislike	people	1.0
Prepare	lesson	1.0
Admire	virtue	1.8
Protect	children	1.6
Starch	white	1.2
Mutton	meat	1.4
Ostrich	feather	1.0
Roof	house	2.0
Little	boy	1.0
Funny	buffoon	2.2
Gay	sun	1.2
Dead	black	1.2
Slow	worm	1.6
Solemnity	feast	1.6
Annoyance	fly	1.0
Constancy	virtue	3.2
Attention	mind	1.4
Uncertainty	pendulum	(12.6)
	Average	1.4

April 27

Stimulus Word	Reaction Word	Reaction Time
Accuse	judge	1.8
Appear	star	2.0
Polish	wood	1.2
Repeat	lesson	1.0
Condemn	delinquent	2.4
Car	motor	1.8
Knee	leg	1.8
Cloud	white	1.2
Fun	joy	1.8
Violent	wind	1.2
Sour	acid	1.0
Dim	sound	1.0
Condition	good	1.0
Deceit	deceive	3.0
Fraud	wrong	3.0
Brutality	animal	2.0
Cup	wine	1.2
Equality	fraternity	3.0
Greasy	pole	1.2
Violet	odor	1.0
	Average	1.7

April 29

Stimulus Word	Reaction Word	Reaction Time
Announce	news	1.2
Stain	ink	1.0
Finish	lesson	1.4
Drag	horse	2.0
Plead	case	2.0
Cork	bottle	2.0
Toy	child	1.2
Key	door	1.2
Ox	horns	2.2
River	water	1.6
Rusty	iron	1.6
Ungracious	bear	2.0
Irksome	science	2.4
Equal	balance	4.0
Late	hour	1.2
Accusation	importation	2.0
Corruption	money	2.0
Poverty	distress	3.2
Imposition	tax	1.0
Adoration	Saint	1.4
	Average	1.8

April 28

Stimulus Word	Reaction Word	Reaction Time
Persuade	argument	2.4
Dig	ditch	1.0
Get	money	1.0
Sting	bee	2.2
Preach	priest	1.0
Spice	pepper	.8
Star	Venus	1.4
Ice	cold	1.0
Picture	beautiful	1.8
Lip	red	1.4
Easy	chair	1.0
Unclean	dirty	1.4
Red	rose	1.0
Rotten	mud	2.0
Hard	flint	1.0
Proposition	geometry	1.6
Improvement	progress	1.0
Infamy	calumny	2.2
Competition (competence)	commerce	2.4
Attraction	actress	2.0
	Average	1.5

April 30

Stimulus Word	Reaction Word	Reaction Time
Adore	Saint	2.2
Perish	ship	2.2
Propose	marriage	1.4
Uphold	politics	2.8
Descend	stairs	1.2
Slave	misery	2.8
Violin	music	2.0
(Path) Pot	country	2.4
Chapel	church	1.4
Trumpet	sound	1.2
Supreme	being	1.2
Elegant	woman	1.6
Impudent	woman	2.0
Blame	offense	2.4
Gain	money	1.0
Idea	noble	1.0
Worship	God	1.0
Elevation	Spirit	1.4
Noisy	metronome	2.0
level	ground	1.0
	Average	1.7

May 1

Stimulus Word	Reaction Word	Reaction Time
Escape	prison	2.0
Admit	argument	2.0
Joke	play	3.0
Improve	mind	1.6
Defy	enemy	1.2
Lamp	fire	2.0
Cabbage	green	1.0
Paste	soft	1.2
Poem	beautiful	1.0
Spear	piercing	2.6
Harsh	sound	1.2
Unripe	fruit	1.0
Unwell	sick	1.0
Vile	fellow	1.0
Admission	employment	3.0
Thankfulness	gratitude	2.0
Dishonor	bad	3.6
Intimacy	friendship	1.0
Revenge	fault	3.4
Least	thing	2.6
	Average	1.9

May 3

Stimulus Word	Reaction Word	Reaction Time
Guide	a traveler	6.4
Care	a boy	2.3
Denounce	principles	3.8
Drop	stone	1.4
Suspect	fault	2.2
Saddle	horse	1.6
Sleep	bed	2.2
Fog	fruit	1.0
Skin	animal	1.4
Earth	ground	3.2
Rough	weather	1.2
High	mountain	1.2
Idle	servant	1.4
Humble	man	2.0
Active	boy	2.4
Health	good	1.4
Aim	noble	1.8
Fame	vain	2.8
Shame	wrong	2.0
Ability	great	1.2
	Average	2.1

May 2

Stimulus Word	Reaction Word	Reaction Time
Deny	favor	2.0
Burn	fire	1.6
Paint	wall	1.8
Betray	faith	1.2
Dress	clothes	1.4
Mouse	black	2.0
Barn	corn	3.0
Song	beautiful	1.4
Spider	feet	2.6
Scarlet	fever	1.6
Beautiful	woman	1.4
Yellow	fever	1.8
Modest	girl	2.0
Wealthy	man	2.0
Justice	right	1.4
Trouble	bad	2.0
Quantity	large	1.6
Reproach	fault	1.2
Energy	force	2.0
Crack	nuts	1.0
	Average	1.8

May 4

Stimulus Word	Reaction Word	Reaction Time
Fast	long	1.4
Dream	sleep	2.6
Taste	food	1.2
Cook	food	1.4
Mark	ink	1.0
Sparrow	bird	1.0
Foot	large	1.6
Spider	insect	3.2
Forest	trees	1.0
Stone	heavy	1.0
Purple	color	1.0
Infamous	calumny	1.2
Refined	art	1.2
Ungracious	bear	1.8
Center	circle	1.6
Awkward	gait	1.8
Supremacy	authority	2.0
Constancy	perseverance	1.6
Time	quick	1.2
Gin	bad	1.0
	Average	1.5

May 5

Stimulus Word	Reaction Word	Reaction Time
Invite	guest	1.6
Pin	clothes	1.4
Tremble (Crumble)	bread	1.4
Attack	enemy	1.2
Wood	hard	2.0
Dirt	nasty	3.2
Shoe	tight	1.2
Camp	large	1.8
Cannon	big	2.6
Ashamed	fault	1.4
Unsafe	war	1.6
Raw	fruit	2.0
Smooth	ground	1.2
Fortune	money	1.4
Disdain	angry	2.0
Refinement	art	1.8
Activity	work	1.2
Accident	misfortune	1.6
Scoff	offender	2.4
Noisy	clock	2.0
	Average	1.8

May 7

Stimulus Word	Reaction Word	Reaction Time
Wash	clothes	1.0
Elevate	thought	1.4
Deceive	wrong	2.6
Ramble	about	1.6
Decay	reign	1.8
Bible	holy	1.4
Pencil	lead	1.0
Crown	king	1.0
Goat	milk	1.2
Candy	sweet	.8
Restless	not quiet	2.0
Simple	countryman	1.6
Reckless	man	1.2
Eternal	life	1.2
Prosperity	fortune	1.0
Jealousy	woman	1.2
Concealment	to hide	2.4
Advancement	progress	.8
Rancid	butter	1.4
Honesty	good	1.0
	Average	1.4

May 6

Stimulus Word	Reaction Word	Reaction Time
Dishonor	sin	2.4
Remove	furniture	1.6
Injure	sword	2.4
Plunge	water	1.0
Murder	thief	1.4
Garden	flower	1.0
Nut	crack	2.0
Stem	heraldry	2.0
Crab	animal	2.0
Pickle	burning	2.0
Noble	gentleman	1.6
Nice	fellow	1.2
Secure	keys	1.0
Blue	sky	2.0
Swift	sparrow	1.4
Disgrace	fault	2.0
Security	policeman	2.2
Unhappiness	marriage	2.8
Rhyme	poetry	1.0
Disaster	Titanic	1.2
	Average	1.7

May 8

Stimulus Word	Reaction Word	Reaction Time
Deserve	merit	1.2
Wish	fortune	2.4
Boast	glory	3.2
Establish	manufactory	1.1
Barber	razor	1.6
Pebble	stone	1.4
Heart	beat	1.2
Machine	work	1.4
Statue	marble	1.2
Certain	thing	2.0
Natural	régime	1.8
Correct	grammar	2.0
Dusty	street	.8
Enormous	building	1.6
Commandment	God	1.0
Excitement	nervous	.8
Restoration	food	1.6
Density	mercury	1.8
Infirmity	sickness	1.8
Return	voyage	1.6
	Average	1.6

May 9

Stimulus Word	Reaction Word	Reaction Time
Paper	write	1.2
Bright	sun	.6
Yellow	fever	1.4
Table	mahogany	3.2
Spoon	food	1.4
Apple	fruit	.8
Sleep	night	2.4
Cut	animal	1.8
Face	beautiful	1.2
Carpet	ground	1.4
Animal	fierce	1.6
Rain	weather	1.8
Teach	lesson	1.8
Doctor	medicine	1.0
Book	interesting	1.4
Store	goods	3.0
Horse	animal	1.6
Island	Malta	1.2
Journey	long	1.0
Freedom	liberty	.8
	Average	1.5

May 11

Stimulus Word	Reaction Word	Reaction Time
Plunge	water	1.0
Guess	enigma	2.4
Rescue	wrecked	1.8
Believe	God	1.4
Carve	wood	1.0
Door	house	1.8
Barley	corn	1.0
Eagle	bird	1.0
Chin	face	1.6
Pulse	beating	1.0
Alive	man	1.2
Exquisite	sweet	1.6
Empty	barrel	1.2
Bitter	quassia	1.8
Lazy	fellow	.8
Modesty	virtue	1.0
Immensity	God	1.6
Preservation	alcohol	1.8
Prudence	virtue	1.2
Indiscretion	vice	1.2
	Average	1.4

May 10

Stimulus Word	Reaction Word	Reaction Time
Roast	meat	1.0
View	panorama	1.8
Whistle	a whistle	1.4
Alarm	people	2.6
Indulge	drinker	1.4
Frost	white	1.4
Cask	wine	1.0
Curtain	silk	1.4
Nurse	baby	1.2
Ivy	wall	1.4
Thankful	grateful	1.0
Steep	stairs	1.2
Unwholesome	air	1.0
Gentle	woman	1.4
Faithful	servant	1.0
Conflict	nations	1.2
Anger	bad	2.2
Idleness	vice	2.4
Betrayal	traitor	1.8
Denouncement	fault	2.0
	Average	1.5

May 12

Stimulus Word	Reaction Word	Reaction Time
Find	treasure	2.0
Praise	merit	2.0
Pump	water	1.0
Try	lesson	1.8
Guard	tower	1.8
Iron	metal	1.8
Stomach	empty	1.8
Salmon	fish	1.0
Bath	water	1.2
Splinter	wood	1.2
Unfit	unable	2.0
Ardent	fire	1.2
North	south	2.2
Handsome	lady	1.2
Price	high	2.2
Appetite	good	1.2
Fable	Aesop	2.0
Definition	grammar	1.8
Queer	sound	2.2
Ingenuity	simplicity	1.4
	Average	1.7

May 13

Stimulus Word	Reaction Word	Reaction Time
Distrust	enemy	1.8
Run	along way	2.0
Agree	friend	1.2
Needle	thread	1.2
Chocolate	sweet	1.0
Twig	tree	1.2
Napkin	white	1.2
Hill	steep	1.4
Finger	hand	1.0
Pretty	girl	1.2
Contented	happy	1.0
Absent	minded	1.8
Magical	lantern	1.4
Profane	words	1.2
Introduction	to a friend	1.4
Amusement	theatre	1.2
Remorse	sin	.8
Calmness	quietness	1.2
Nod	head	1.0
Calculate	Numbers	1.0
	Average	1.3

May 15

Stimulus Word	Reaction Word	Reaction Time
Sin	bad	1.4
Applaud	merit	.8
Astonish	marvel	1.6
Rejoice	good news	2.0
Use	tools	1.2
Spool	loom	1.4
Sheep	fur	1.6
Emerald	precious stone	1.8
Wagon	coal	1.6
Cottage	college	1.6
Naughty	boy	1.2
Exacting	demand	2.6
Thirsty	man	1.2
Playful	boy	1.2
Impulsive	dashing	1.8
Faithfulness	dog	1.0
Provocation	insult	1.4
Contentment	happiness	1.0
Religion	faith	1.0
Profanity	bad word	1.0
	Average	1.4

May 14

Stimulus Word	Reaction Word	Reaction Time
Shock	electricity	1.4
Sweat	heat	1.8
Melt	snow	1.4
Stun	hit	1.4
Hunt	deer	2.0
Maiden	woman	1.8
Bag	sand	2.0
Belt	leather	1.2
Cake	sweet	1.2
Unhappy	miserable	1.6
Pure	blood	1.8
Disorderly	irregularity	1.6
Unemployed	poor	2.0
Wretched	miserable	2.0
Indulgence	vice	1.6
Agreement	friendship	1.2
Advantage	benefit	1.2
Injury	blow	1.2
Outrage	war	1.6
Rubber	teeth	1.6
	Average	1.6

June 2, 1913

Stimulus Word	Reaction Word	Reaction Time
Adore	God	1.0
Perish	ship	1.0
Propose	marriage	1.1
Uphold	opinion	1.8
Descend	mountain	1.2
Slave	poor	2.2
Violin	song	1.6
Brook	river	1.0
Chapel	church	1.0
Trumpet	sound	1.0
Supreme	Being	1.2
Elegant	lady	1.2
Impudent	boy	2.8
Blame	fault	2.1
Gain	money	.8
Idea	beautiful	1.4
Worship	God	1.6
Comfort	pleasure	3.0
Noisy	room	1.0
Level	ground	1.0
	Average	1.4

June 3

Stimulus Word	Reaction Word	Reaction Time
Cover	hat	1.3
Hasten	pace	1.0
Curse	son	3.6
Hurt	wound	1.4
Blush	young lady	2.2
Island	Malta	.8
Copper	mental	1.0
Water	flowing	1.0
Lettuce	vegetable	1.4
Brandy	alcohol	1.0
Unseen	God	1.0
Merry	happy	1.6
Sacred	church	1.4
Excellent	exam	1.6
Adorable	Saint	1.4
Life	Eternal	1.2
Opposition	enemy	1.2
Intellect	mind	1.2
Sorrow	grief	1.4
Education	school	1.2
	Average	1.4

June 5

Stimulus Word	Reaction Word	Reaction Time
Oppose	enemy	1.2
Enter	house	1.2
Drive	horse	1.0
Lecture	public	2.2
Flag	wave	1.0
Ivory	white	1.0
Bed	sleep	1.2
Fountain	water	1.0
Pie	lemon	1.6
Awake	morning	1.4
Dull	night	1.4
Many	friends	1.8
Green	leaves	1.2
Divine	God	1.0
Terror	enemy	1.2
Spite	hatred	1.4
Advice	council	2.0
Contempt	enemy	1.8
Dispute	question	1.2
Telephone	friend	2.6
	Average	1.4

June 4

Stimulus Word	Reaction Word	Reaction Time
Caress	baby	1.4
Reduce	salary	1.0
Reward	behavior	1.8
Talk	English	1.0
Touch	table	1.0
Street	long	1.0
Cane	reed	1.2
Soap	soft	1.4
Cheese	English	2.0
Drum	sound	1.0
Happy	healthy	2.2
Small	boy	1.0
Difficult	lesson	1.2
Painful	wound	1.2
Grief	sorrow	1.0
Thought	good	1.4
Credit	great	1.6
Fear	death	1.4
Mercy	God	1.2
Sinful	man	1.0
	Average	1.3

June 6

Stimulus Word	Reaction Word	Reaction Time
Scold	child	1.0
Walk	street	1.0
Punish	criminal	2.2
Smell	odor	1.2
Send	letter	1.4
Mill	flour	1.0
Elbow	hand	1.2
Milk	white	1.0
Scissors	cut	1.2
Moon	night	1.2
Quiet	night	1.4
Infinite	God	1.0
Brave	soldier	1.4
Ornamental	church	1.0
Dreadful	fight	1.4
Chance	good	1.4
Quarrel	men	2.0
Conscience	good	1.2
Scandal	bad	1.8
Evil	bad	1.6
	Average	1.3

June 7

Stimulus Word	Reaction Word	Reaction Time
Irritate	nerves	1.0
Tame	animal	1.0
Feed	animal	1.2
Imagine	vision	1.0
Suffer	pain	1.0
Dinner	good	1.2
Raft	sea	1.2
Chart	fever	1.8
Glove	hand	1.0
Bird	sing	1.2
Afraid	lion	1.0
Blue	sky	.8
Anxious	desirous	1.2
Long	street	1.0
Audacious	hero	1.2
Expression	vocal	1.2
Mistake	great	1.2
Devotion	church	1.2
Errand	boy	1.0
Expense	great	1.4
	Average	1.14